Psychophysiological Mechanisms of Hypnosis

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With 50 Figures

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Foreword

It seemed appropriate for the First International Symposium on the Psychophysiological Mechanisms of Hypnosis to be held in France, the country where hypnosis was rediscovered with the work of MESMER and where somnambulism was first described by his student, the MARQUIS DE PUYSÉGUR. The classic studies of such authorities as CHARCOT, JANET, BERNHEIM, BINET, and FÉRÉ, as well as many lesser known French scholars, have provided many of the concepts that remain significant to this day.

There have been many periods in the history of hypnosis when outstanding members of the scientific community became intrigued and fascinated with the phenomenon, only to lose interest and allow it to become the province of the charlatan and fiction writer. The Second World War provided considerable impetus to the clinical use of hypnosis and since then there has been an enduring revival of both scientific and clinical interest. Nevertheless, though research using hypnosis or attempting to clarify its nature has become recognized as an appropriate scientific endeavour in much of the world, a great many barriers of communication, including not only those of language but, perhaps even more troublesome, those of theoretical orientation and disciplinary interest, have remained a serious impediment to progress. Papers dealing with hypnosis have been presented at many international meetings and, in recent years, two international meetings have been devoted specifically to this topic; yet there has been relatively little opportunity for a detailed exchange of views or for meaningful efforts to understand and resolve differences. The idea of a small meeting limited to active workers in the field who might get together for several days away from the distractions of their day-to-day work — or the attractions of a metropolis — was first conceived several years ago in informal discussions between Dr. CHERTOK and myself. It was intended that the participants include as broad a range of disciplines and as wide an international representation as possible while sharing the interest in different aspects of the hypnotic phenomenon.

Drs. CHERTOK and BUSER, who were the organizers of the meeting, obtained the sponsorship of the International Brain Research Organization, the only international scientific group representing a range of interests broad enough to encompass all the scientific disciplines that were to be represented at the Symposium. The meeting would not have been possible without the sponsorship and generous support of the Ligue Française d'Hygiène Mentale, the Institute for Experimental Psychiatry, and the U. S. Office of Naval Research, Group Psychology Branch.

After considerable preparation, in the fall of 1967 in a beautiful setting at Tourtour in the south of France, the First International Symposium on the Psychophysiological Mechanisms of Hypnosis was held. As in all symposia, there were many outstanding colleagues who we wish could have attended but whose participation was precluded by either their own commitments or limitations of support. Every effort was made to bring together participants who differed in background and orientation but shared a research interest in and experience with hypnosis.

The participants met both formally and informally over the five-day period. While formal papers were presented, ample opportunity was allowed for detailed discussions. It was the conviction of all the participants that the sessions not only were stimulating and exciting but served to bridge many of the differences in orientation, training, and points of view. It was again demonstrated that colleagues from different countries and different disciplines, with widely different theoretical predilections, can work together meaningfully on a problem of common interest. At the final session, all participants felt that a second such meeting should be held at a future date. We wish it might have been possible to publish some of the exciting discussions that took place. Since there was no feasible way of arranging and editing these discussions, the participants were encouraged to formalize and update their manuscripts and to include, when appropriate, some of the material that had emerged from the discussions, thus making available much of the content of the Symposium.

Dr. CHERTOK assumed the difficult and demanding task of both riding herd on those of us who were slow in making our manuscripts available and, when necessary, assuming responsibility for the editing of papers. Both the meeting and this volume are tributes to the enthusiasm, persistence, and organizational skills of Dr. CHERTOK.

Speaking for all who attended, we hope that these papers will help colleagues in many fields to see the relationships between their own interests and those of others separated by disciplinary or national boundaries. To the extent that this aim is accomplished, the First International Symposium on the Psychophysiological Mechanisms of Hypnosis will have been successful.

Philadelphia, July 1969 MARTIN T. ORNE, M. D.

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Introduction

The Evolution of Research on Hypnosis. Introductory Remarks

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It is indeed gratifying that, in taking place in France, this Symposium renews in a certain sense the links with the great debates which occurred in this country in the last century, and were rendered particularly illustrious by their association with the names of CHARCOT and BERNHEIM. It should be added that this brilliant period was unfortunately followed by a prolonged eclipse, and it is only in the past few years that a renewed interest in hypnosis has manifested itself in France.

History

An examination of the history of hypnology in the past two centuries reveals that, far from showing a continuous evolution, it experienced many periods of stagnation. This is to be explained by the fact that, in this science more than in any other, there have always existed specific epistemological obstacles, in consequence of the element of passion that accompanied all discussions on this subject. Moreover, the investigator finds himself involved in a relationship with the patient which gives rise to affective reactions.

This situation had already been clearly demonstrated as early as 1784, in the famous Reports by the members of the Royal Commissions appointed by Louis XVI to investigate animal magnetism. In fact, two public Reports, and one "secret" Report, were compiled (BAILLY, 1784a, b, c). The former condemned animal magnetism, on the grounds that the mesmeric fluid did not exist and the effects of magnetism which they noted were entirely attributable to the imagination. Thus, in these documents, which were based on the most scrupulous observations, the members of the Commissions, while officially concurring in the rejection of "fluidism", at the same time, albeit unwittingly, affirmed the reality of psychological action and its physiological effects. It can be said that, with the work leading to these Reports, hypnosis—under the name of animal magnetism—for the first time became the object of experimental enquiry. But simultaneously, the compilers of the Reports had adopted an attitude which was for a long time to divert investigators from undertaking any more detailed research in this field. Indeed, their secret Report denounced the moral danger that the practice of animal magnetism might entail. This caution reinforced unconscious resistance, with the result that for some fifty years the actual existence of hypnosis was still disputed, until, in 1840, the learned bodies in France finally decided to shelve the question (BURDIN and DUBOIS, 1841).

A new era was opened up in Great Britain by BRAID (1843), who introduced an original technique of hypnotism—and one which was more objective, in that the hypnotic passes were replaced by visual fixation on a bright object. This more impersonal technique, associated as it was with an explanation in neurophysiological terms (which was in fact purely metaphorical), produced a reassuring effect. BRAID's work was further pursued in France by LIÉBEAULT (1866) who, in addition, together with BERNHEIM, disseminated widely the technique of verbal suggestion. Although these investigators both interpreted verbal suggestion in physiological terms, they none the less came under attack on the part of the Salpêtrière School, who sensed that a personal factor was involved in suggestion. CHARCOT (1882) evolved a purely somatic theory of hypnosis, divorced from all suggestive concepts—a theory which, associated with the considerable prestige attached to the master of the Salpêtrière, reassured the practitioners and, as we know, ensured the further development of hypnosis. It should moreover be noted that, with CHARCOT's death in 1893, his theory suffered a major set-back, and it was then that hypnosis entered on a period of eclipse, more or less prolonged according to the country, but which in France persisted until not many years ago.

It is generally stated that, in the conflict in which they were protagonists, the Nancy School ultimately triumphed over the Salpêtrière School (CHERTOK, 1967). But the very fact that the former was psychologically oriented once again aroused strong resistance, and its "victory" was followed by a quasi-neglect of the study of hypnosis. In France, with the exception of JANET's work, it was hardly followed up at all. In Russia, however, a small focus of interest persisted, particularly under the impetus provided by PAVLOV, who accepted suggestion, albeit supporting it with a physiological theory based on new concepts. Such a theory, propounded on the authority of so outstanding a scientist, exerted (as had also been the case with CHARCOT) a reassuring effect.

Certain investigations had already been undertaken in the United States between the two World Wars (HULL, 1933); but after the Second World

War a new interest in hypnosis was awakened by the work of ERICKSON (1967), WOLBERG (1948), BRENMAN, GILL, KLINE, SCHNECK and others (cf. references in CHERTOK, 1963), and it is since then that research on hypnosis has made the most appreciable progress. These advances are related to various investigations in experimental psychology, and more particularly those associated with the Schools of HILGARD and of ORNE, both of whom are present amongst us today. These investigators, seeking to quantify hypnotizability, had of necessity to work with large groups, and in this way, to employ such techniques as hypnotic induction by tape-recorder, or by the reading of standardized texts, etc. It was a matter of reducing so far as possible the interpersonal factor, and this attempt at objectification no doubt helped to undermine the usual resistance towards the study of hypnosis.

Prospects

In the light of this rapid historical outline, it may perhaps be justifiable to state that, after various vicissitudes, research on hypnosis is at the present time undergoing some degree of revival, to which our Symposium in its way bears witness. There is ever-increasing recognition of the importance of hypnosis for the understanding of many facts pertaining to psychophysiology, as also to normal and abnormal psychology.

With regard to psychopathology, we know what interesting perspectives have recently been opened up by the studies on REM sleep, which has been described as *a third organismic state*, additional to the waking state and sleep. Might one not regard hypnosis as *a fourth organismic state*, albeit one that is unfortunately difficult to circumscribe and to objectify, particularly by means of recordings of electrical activity. But its lability is itself valuable, in so far as it may provide information as to the manner in which the passage from one state of consciousness to another is effected, not only in neuroses and psychoses, but also in individuals who are subjected to fundamental variations in their environment (astronauts, deep-sea divers, spelaeologists), or to the mass media of propaganda or advertising. I shall return to this point later.

This being so, and by reason of all the avatars which I have mentioned, hypnology remains a very young science, and one that is pervaded with uncertainties. Hypnosis is not yet the object even of a satisfactory definition. It is generally conceded that it constitutes a specific state, but the actual element, whether physiological or psychological, which goes to make up its specificity has not yet been precisely determined. This problem has been approached from various angles, inspired, respectively, by physiology, experimental psychology and psychoanalysis. I would like to give here a brief summary of these several aspects in very broad perspective.

Physiology

The physiological theories have always been centred on the relationship between hypnosis and sleep (LIÉBEAULT, 1866; BROWN-SÉQUART, 1882; BEAUNIS, 1887), hypnosis being regarded as an incomplete sleep. For the Pavlovians, it is a partial cortical inhibition with continuance of "waking points" in certain areas, thus allowing communication between the subject and the operator. They point out that, even in normal sleep, a selective relationship may be established—as in the case of a mother who, fast asleep, is awakened by the feeble cries of her baby.

According to the Pavlovians, the existence of "waking points" is confirmed by an experiment in animal physiology: a dog conditioned to the sound of a trumpet associated with the appearance of its food, awakens only at this sound, and remains insensitive to other, even more intense, sounds (BIRMAN, 1925).

Apart from the difficulty of applying to man the results of experiments undertaken on animals—particularly the fact that language cannot possibly be compared with a physical stimulus, as PAVLOV himself admitted—it should be emphasized that the hypothesis of the similarity between hypnosis and sleep has received no electroencephalographic confirmation whatsoever (CHERTOK and KRAMARZ, 1959). This does not exclude the possibility that a certain functional relationship, which has not yet been physiologically objectified, exists between the two states. It is the view of some psychoanalysts (BRENMAN, 1951; BELLAK, 1955) that there may exist such a relationship, of a psychological order.

Another line of physiological research is represented by the work on animal hypnosis, also known as shamming death, catalepsy, akinesis, etc. (CHERTOK, 1964). Some authors are reluctant to use the term, "animal hypnosis", so as not to postulate any identity with human hypnosis.

Others, however, such as SCHILDER—an eminent scientist, who was a psychologist as well as a neurologist—have not hesitated to establish such a connection (SCHILDER and KAUDERS, 1926). There exists, in any case, in both forms of hypnosis a common factor, i.e., motor inhibition, which is constant in animals and frequent in man. In the absence of physiological signs characteristic of hypnosis, this specific manifestation is certainly deserving of study in the perspective of basic research. Might it not be possible here to adopt the same approach as in the case of dreaming sleep (REM)—another phenomenon common to man and animals, where the difficulty of transposition does not prevent the extensive use of observations made on animals?

Experimental Psychology

Coming now to the subject of research in experimental psychology, I must state at the outset that I shall only refer to it very sketchily, in view of the considerable increase in investigations in this field in recent years. I shall leave it to the distinguished representatives of this discipline who are here amongst us, to enter in due course into greater detail. The concepts of BERNHEIM (1884) were again taken up by experimental psychologists in the United States, and notably by HULL (1933), in the thirties. They made a particular study of suggestibility, and confirmed that this constituted the whole of hypnosis, which thereby almost lost its specificity. Hypnosis was regarded as a kind of learning, but little by little, motivational factors were taken increasingly into consideration. It has now come to be generally recognized that suggestibility does not entirely explain hypnosis. Attempts to quantify the latter were made by means of increasingly ingenious techniques, amongst which should be mentioned those of WEITZENHOFFER (1953), HILGARD, and ORNE. The last was led to distinguish an "essence" of hypnosis, and "artifacts" which would be the result of the socio-cultural influences of a given period and of the elements communicated consciously or unconsciously by the hypnotist. ORNE (1959) believes that by eliminating all "artifacts", we shall arrive at the true "essence" of hypnosis.

HILGARD (1965) has studied the psychological characteristics of hypnosis, of which, according to him, there are seven: subsidence of the planning function; redistribution of attention; availability of visual memories from the past, and heightened ability for fantasy-production; reduction in reality testing, and a tolerance for persistent reality distortion; heightened suggestibility; aptitude for role behaviour; and amnesia for what transpired within the hypnotic state. HILGARD has particularly concentrated on the problem of hypnotizability, from the point of view of quantification. But he admits that this approach is not sufficient to provide an answer to the fundamental question: why is one individual more readily hypnotizable than another? In this respect, he recognizes the need to take into consideration the subject's history, his potential regressions, and his deeper motivations. He thereby initiates a dialogue with the representatives of depth psychology.

Psychoanalysis

The psychoanalytical view on hypnosis has also undergone considerable modification in the past few years. Originally, the hypnotic situation was interpreted as a gratification of the subject's instinctual wishes. It was seen as a masochistic type of relationship and a form of transference (FERENCZI, 1909). Subsequently, in addition to the libidinal impulses involved in hypnosis, the somatic, sensorimotor dimension was taken into account. Thus KUBIE and MARGOLIN (1944) stressed the idea, supported by their own experiments, that the hypnotic state can be induced by physical manipulation without the presence of the hypnotist, i.e., without transference. The transference, when it occurs, is the result and not the cause of the hypnotic state. These two authors at the same time attempted a synthesis between psychoanalytical and Pavlovian physiological theories. Their hypotheses, which were not in fact followed up by experimental research, interpreted on the psychological plane the physiological process described by PAVLOV: the formation of a concentrated zone of cortical excitation, surrounded by a zone of inhibition, would correspond to a dedifferentiation of the ego and the external world, represented by the hypnotist—a process in which a fusion of subject and hypnotist is achieved. The subject undergoes an infantile regression, with the hypnotist fulfilling the role formerly played by the parents.

In 1954, the work of Hebb's school on "sensory deprivation" gave some degree of support to KUBIE's and MARGOLIN's hypotheses about the somatic dimensions of the hypnotic state. It showed, indeed, that physical, sensorimotor manipulation could give rise to changes in the mental state. GILL and BRENMAN (1959) have drawn upon both the work of KUBIE and MARGOLIN (1944), and that of Hebb's school, in formulating a theory of hypnosis oriented towards the ego-psychological trend in psychoanalysis. This trend, as is known, endeavours to relate research in the fields of physiology, psychology and psychoanalysis. In the view of GILL and BRENMAN, there is a kind of interdependence between "sensorimotor" and "transferential" factors. For them, "hypnosis is a particular kind of regressive process which may be initiated either by sensorimotor-ideational deprivation or by the stimulation of an archaic relationship to the hypnotist". They add that "when a regressive process has been set into motion by *either one* of these two kinds of factors, phenomena characteristic of the other kind begin to emerge".

Although the work of KUBIE and MARGOLIN served as a starting point for GILL and BRENMAN, there remain between these respective authors appreciable divergences of opinion. Without mentioning all of these here, I will nevertheless refer to those which relate specifically to the questions of transference and regression. For GILL and BRENMAN, the transference is a constituent element of hypnosis. For KUBIE (1961), it is only an epiphenomenon which may or may not appear. As to regression, it is interpreted by GILL and BRENMAN as a decrease of autonomy of the ego with respect to the id (regression in the service of the ego). But KUBIE sees it as the end-result of a whole series of "dynamic" mechanisms, as much on the conscious, preconscious and unconscious levels, as on the psycho-neuro-physiological plane. In fact, KUBIE does not believe that there is anything specific about hypnosis on a purely psychological plane. What is specific, and what has to be understood, is the psycho-physiological setting within which the hypnotic process unfolds. The crucial point for him is the study of the "transitional processes" and "trigger-mechanisms" that set off these alterations in the state of consciousness. He consequently regards hypnosis as a very special instrument of research, since it can induce these alterations experimentally. KUBIE goes so far as to assert that, when its mechanism is understood, it will provide "one of our most important tools for the study of normal sleep, of normal alertness, and of the continuous interplay among normal, neurotic, and psychotic processes".

Conclusion

The three lines of approach which have been summarized above—physiology, experimental psychology and psychoanalysis, whatever fragmentary data they may have supplied, have not so far enabled us to arrive at a true understanding of hypnosis. Research has been undertaken separately in each discipline, but it may well be asked whether it is not now time to initiate a dialogue between research workers. Such a dialogue, needless to say, implies, in the first place, that each discipline should have reached an adequate level of development. But it also presupposes that the investigators themselves are prepared to collaborate, and to recognize that hypnosis—a complex reality, psycho-physiological in its nature—can hope to receive a valid explanation only as a result of collective research comprising all its aspects.

The discussions which are about to take place will, I hope, provide the opportunity for commencing this interdisciplinary dialogue. It is, however, to be feared that the representatives of the various disciplines who will contribute to the Symposium will have some difficulty in reaching an understanding, not only because of divergences in their approach, but also, and especially, owing to the fact that they will be employing different languages, according to their speciality. In any case, whatever the difficulties that may confront us, I believe that we should make every effort to overcome them and engage in a fruitful dialogue. What is at stake surely makes this worth while.

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I. Physiology

Some Physiological Mechanisms Amenable to Control by Direct Suggestion under Hypnosis

STEPHEN BLACK, Psychophysiological Research Unit, London

I. Neurological

The phenomena of hysterical conversion in clinical neurology indicate that both the sensory and motor functions of the central nervous system are almost totally amenable to control by psychic processes — and that given the appropriate psychopathology, anaesthesias and palsies can appear without any evidence of organic disease or trauma. In research on the relationship between mind and body, it is therefore of some significance that literally all such hysterical conversion symptoms of a neurological nature can be precisely reproduced by direct suggestion under hypnosis (DSUH) in entirely healthy, deep-trance hypnotic subjects.

There is to date, however, no satisfactory explanation as to how such effects take place and although the clinician may distinguish between the glove and stocking anaesthesia of conversion states and the organic anaesthesias—by pin-prick analysis of the distribution—such diagnosis contributes nothing to our understanding of the psychophysiology involved. Indeed, in the absence of any clearly-defined theory as to the nature of "mind", it is difficult to see in what terms such an explanation could be rendered.

I have recently published a theory on the nature of mind (BLACK, 1969) — but theory aside, it can be concluded with some certainty that the mental process *per se* is manifest by the receipt and transmission of information. In general, therefore, mind has the qualities of an "informational system" as originally defined by HARTLEY (1928) — and in particular, "information" itself has the qualities defined mathematically by WIENER (1948), SHANNON and WEAVER (1949), and BRILLOUIN (1956).

In these terms, an "informational system" can be broken down into "source", "channel", and "receiver"—with possible "feed-back" from receiver to source. And "information" itself becomes "the logarithmic function of a probability". From this it then follows that the more improbable an event, the more information it contains—and conversely, there is no information to be extracted from events of high probability.

If, then, anaesthesias and palsies can be created by "mind", it can be postulated that, instead of causing physical interference with the electrical system of brain and nerves—of which there is no evidence in the form of organic change, such phenomena are produced by interference with the "anatomy of information itself". More precisely, it can be proposed that what is altered in these circumstances is the probability function on which all such information must be based.

Thus the amount of information recorded by a stimulus becomes related to some previously recorded probability in the relationship between the stimulus and the environment. In the cerebral mechanisms by which information is then extracted from the sense data of experience, some system is likely to be included, by which such probabilities are assessed; and the appearance of palsies and anaesthesias would then depend on the failure of this system. The experiments reported here were originally designed to investigate such an hypothesis.

The scientific view of the world, as THORPE has now written (1969), is a "deliberate restriction to certain areas of our total experience—a technique for understanding *certain parts* of that experience and achieving mastery over nature" (my italics). Obviously the number of associations recording individual history—and for all we know, phylogenetic history as well—must be very large indeed and most of them unavailable for experimental control. But taking a view similar to that of THORPE, as I saw it in 1960, one important relationship of any stimulus by which its "probability" and thereby its "information content" can be assessed, must be the simple probability of its association with another stimulus, whether in the same or in a different sensory modality.

I postulated that on this basis controlled experimental conditions could be set up which might render just such an association as something approximating to the "restricted area of experience" I was seeking. We see the lightning and await the thunder; but whatever the large number of associations between "thunder and lightning" as recorded in the total memory—and even the Jungian unconscious—in the event of a thunderstorm, it is this momentary association between visual and auditory stimuli which must count in the „assessment of probability" and the eventual extraction of information from the sum of accumulated data.

There had, of course, already been many attempts to describe and investigate the physiology of the central nervous system in terms of informatiion theory, and by 1960 a substantial body of work in this field had already been reported by such authors as MACKAY (1957a; 1957b; 1960), BARLOW (1958, 1961), POLLACK (1961) and others. In particular, the *adaptation* of sensory end-organs described by ZOTTERMAN (1959), the *accommodation* of peripheral afferent nerves reported by HILL as early as 1936, and the cortical *habituation* of HERNANDEZ-PEON (1961), all indicated that at least *some* assessment of probability is already in progress right from the peripheral source of stimulation through to the cortical areas of specific sensory projection.

Since, in this way, nerve tissue from end-organs to specific cortex appeared in general to respond to probability, and since impulses arriving at specific cortical areas obviously have to be relayed again before reaching motor cortex, it seemed likely that the probabilities involved were subject to even further processing before activation of the pyramidal cells. But the question then was: where, in the total complex of brain tissue, does such an ultimate assessment of probability take place?

In 1949, MORUZZI and MAGOUN had made the highly significant discovery of a collateral afferent system originating in the mesencephalon and diencephalon—in effect, the midbrain-thalamic structures of the "reticular system"—by which nervous impulses effected by peripheral stimuli are relayed to the cortex in "parallel", as it were, with the projection fibres of the other afferent pathways. And as a result of this "reticular projection", peripheral stimuli had been found to achieve a much more widespread cortical activation than was previously supposed. In particular, it was shown that, apart from potentials evoked in appropriate regions of specific cortex—in visual, auditory and other sensory areas—in the presence of any form of stimulus, potential changes could also be recorded from the anterior lobes of non-specific cortical tissue —or what had until then been described as "the great silent areas of the brain".

Important technical developments in the field of electroencephalography (EEG) then made it possible to obtain electronic averages of any cortical potentials as recorded with electrodes on the scalp, or with electrodes implanted in the brain. As a result, the study of *averaged* EEG records in man and animals opened up a new field in electroneurophysiology (DAWSON, 1951; COOPER and WARREN, 1961).

But whatever the value of these practical achievements to the study of intrinsic cortical rhythms—such as the alpha rhythm and even aberrant rhythms of pathological origin, they were inevitably destined to make their greatest contribution in the field of evoked potentials, because of the essentially transient nature of such responses. For if electronic averages could be taken and the stimulus repeated many times, however transient the response, the *average* provided a record which was not only more clearly defined than the single record, i.e., had a lower noise-message ratio, but was statistically more significant according to the number of stimuli presented.

Now in general the magnitude of the evoked potential difference of the specific cortical response is a function of the intensity of the stimulus: in visual cortex, a brighter light evokes a larger response than a dim one, and in auditory areas a faint sound evokes only a small potential change, while a loud sound evokes a large one. But working on the evoked responses of the reticular projection to anterior non-specific cortex, WALTER (1963) showed that over wide areas from supra-orbital regions to vertex, these were not only diminished by monotonous repetition as implied by the work of HERNANDEZ-PEON and others (HERNANDEZ-PEON and BRUST-CARMONA, 1961), but that they tended to have an irregular and sometimes even an inverse relationship to stimulus intensity.

Investigating this further, WALTER then showed that a weak stimulus which is unexpected is likely to evoke a larger response than a strong one to which the subject is accustomed. In effect, whatever the magnitude of the non-specific cortical response as related to stimulus intensity, this could be over-ridden by the mechanisms of habituation. From WALTER's work it seemed, therefore, that the ultimate process of probability assessment in the total chain of events from stimulus to the motor function of "behaviour", could well be a function of those otherwise unexplained tissues of the frontal lobes.

On this basis, WALTER and I set out to investigate the effect on such anterior non-specific cortical responses of an association in time between stimuli in different sensory modalities. The subject was presented at *irregular* intervals—irregular, to avoid the effects of habituation—with a click, which was always followed regularly in time by a flash; and recordings of averages were made with electrodes over various areas of the anterior cortex.

As could be postulated from first principles, we found at once that while the response to the click was maintained — since it was irregular, and thus improbable and therefore contained information—the response to the flash was rapidly diminished after only a few presentations; for once the click had occurred, the flash acquired an increasing probability with every presentation. And *ex hypothesi*, it therefore contained less information.

The averaged record obtained after, say, 12 presentations, was thus a negative-positive pair of spikes representing the response to S_1—the click. These were followed, in the precise period of time chosen, by a small positive spike recording S_2, i.e., the flash to which habituation had been established. Since this habituation of S_2 was contingent upon S_1, we described the dual association between S_1 and S_2 as "contingent habituation".

Among other things, it was clear that the diminished response to S_2 was evidence that the tissues involved were able to retain—at any rate over a period of, say, one second—the information that following a click there was very likely to be a flash. Thus some form of "memory" was involved, and the problem then was to design a modification of the association which would not only demonstrate the existence of such "information storage", but would bring to light its dependence on the probability of association between S_1 and S_2.

I argued that information could not be "stored" unless it had been received. And from this it followed that interference with the basic "probability of association" under precise experimental conditions should make it possible to manipulate the magnitude of the response to S_2 at will.

Now it is well known that the hypnotized deep-trance subject will report "seeing", "feeling", and in this instance, "hearing" almost anything that the hypnotist likes to suggest. And although there was, to date, no objective evidence as to the "reality" of such experiences—they were only reported by the subjects, there was much to suggest from descriptions given at the unconscious level under hypnosis, that subjectively at least, such hallucinations were indistinguishable from reality.

I concluded that in the course of recording averages of anterior cortical potentials evoked by associated real clicks and flashes, DSUH of hallucinated clicks should dilute the probability of such an association—and should thereby reduce the information content of the real clicks. If this happened, as I postulated, the real clicks would then convey minimal information for memory storage before the real flashes arrived. And since the real clicks were to be presented at irregular intervals, to maintain their information content, the flashes would be equally irregular and would consequently "gain in information" and the magnitude of the responses they evoked would be increased.

In the experimental design which was developed from these theoretical ideas, the subject lay on a couch wearing high-fidelity headphones and facing a xenon flash tube. By electronic control, a series of 12 clicks at mathematically irregular intervals was presented in the headphones—and at an interval of one second following each click, light flicker at 15 cps was presented from the xenon tube.

The subject was told beforehand to act on the appearance of the flash by pressing a bell-push button—the required motor response representing the ultimate evidence of "behaviour". But it was explained that the apparatus was so designed that if the button were pressed too soon—pressed before the appearance of the flash—no flash would appear. In many respects, therefore, this operant motor response was related to the flash as in experiments on reaction times. Recordings of the evoked potentials were then made from wide areas over the anterior non-specific cortex, and the average of 12 presentations was taken from 2 channels using an Electronic Average Response Indicator (EARI) designed by COOPER and WARREN (1961).

The first thing to do was to establish contingent habituation of the associated stimuli, i.e., to produce an average of 12 presentations of the associated clicks and flashes which showed a clearly defined response to the irregular clicks and minimal response to the associated flashes. This was, then, the essential "research tool" with which we would be working.

The subject was next hypnotized into a deep trance and DSUH given of hearing "at irregular intervals of about one or two per second, a series of clicks identical with those already heard in the headphones". A further series of 12 presentations was then made and a second average taken.

The result of the first experiment elegantly supported the various hypotheses. Although contingent habituation was clearly present *before* the suggestion of hallucinated clicks, following DSUH of "hearing clicks" the negative component in the response to the flashes increased to maximum dimensions, while the negative component in the response to the real clicks was slightly diminished.

As every experimental physiologist knows, it is only very rarely indeed that hypothesis so accurately predicts an experimental outcome. Whatever else might be going on, it seemed clear that the magnitude of the negative component in the responses to the flashes, was literally dependent on the probability of its association with the clicks —and thereby on the probability of the clicks themselves.

Entirely subjective dilution of this probability had effectively diluted the probability of the association, and although objectively there had been no change in this probability, it could no longer be recorded by the brain and there was therefore "no information available" for storage. As far as the subject's anterior cortex "was concerned", the flicker to which a motor response was demanded, came unpredictably at irregular intervals in the midst of a confusing multiplicity of clicks, both hallucinated and real. As might be expected under such circumstances, the reaction time was therefore greatly increased as well.

Publication of these results (BLACK and WALTER 1965a) provided not only the first objective evidence at the cerebral level of the "reality" of the hallucinated experience, but it was also the first objective evidence of one way at least by which the brain responds to hypnotic suggestion: for here at last were EEG records of a cerebral mechanism responding to hypnotic suggestion. It seemed, as postulated, that at least one of the functions of those "great silent areas of the brain", was in fact to act as a "contingency computer" and to extract information from the environment by the assessment of probability.

It was soon shown, of course, that the probability of association between S_1 and S_2 could be equally well manipulated by dilution with real, unassociated clicks. And we were thereby able to determine the "degree of dilution" and to show with some precision when comparing stimuli of equal intensity, that the magnitude of the evoked negative potential in non-specific cortex is an *inverse function* of the probability of the stimulus.

Thus, if the degree of dilution of 12 presentations of associated stimuli was carried out with only 3 additional and unassociated clicks, giving 12/15 ($p = 0.8$), the increase in size of the negative variation in response to S_2 was minimal. But it increased rapidly in size when the dilution was raised to 6 additional and unassociated clicks, giving 12/18 ($p = 0.6$), and reached the maximum attainable at any given intensity with 12/24 ($p = 0.5$), These odds are, of course, "evens" in betting terms—which means in effect that no information is available prior to the actual presentation of S_2 and there can therefore be no assessment of probability in relation to S_1 which is of any value to the organism.

Diluting with hallucinated clicks, I have since been able to predict, from the magnitude of the negative variation in response to S_2 as measured in μV, the actual number of hallucinated clicks being heard by any particular subject, in the course of 12 presentations with real clicks and flashes. Indeed, the evidence of these objective observations entirely supports the subjective claim of the hypnotized individual that the hallucinated experience is identical with reality.

Observation of these phenomena did not, however, provide any explanation of what was going on. And the question still remained as to why cortical tissue should respond in this way. In the time lapse between S_1 and S_2, some kind of "memory mechanism" had to be involved, however transient, and the nature of this mechanism seemed to suggest a fruitful line of enquiry.

Our recordings had been made with an EEG time-constant of 0.3 sec. But examination of the prime records showed that in the presence of contingent habituation, between S_1 and S_2 there was nearly always a slow, negative swing which was then cut short by return to the neutral position. To investigate this, experiments were set up with directly coupled amplifiers.

Such "DC recording" is a complicated business—in the first place, all the electrodes involved have to be equipotential and must be processed beforehand to achieve this. But WALTER having mastered the various technical difficulties involved, the first clean records obtained seemed to provide an answer, and actually indicated the nature of that "transient memory" between the paired stimuli.

In the presence of contingent habituation, there was seen to be a persistent, slow negative variation immediately following the positive spike at the end of the response to S_1. Throughout the following second, or whatever time-lapse was allowed before the presentation of S_2, this negative swing continued to increase in magnitude, but on the presentation of S_2 it was at once converted into an abrupt positive spike, followed for some milliseconds by a general process of oscillation about the neutral point.

The experiment was, and indeed has since been, repeated many times with a variety of paired stimuli in most sensory modalities, and the results are entirely consistent with our early findings. In effect the association in time between two stimuli was found to produce a transient "DC shift" in which the usual negative variation in response to S_2 apparently became engulfed—as this might be recorded with a 0.3 sec time constant. Recording with directly coupled amplifiers showed that this process of cortical depolarization only occurred in the presence of an expectancy of association, and that the magnitude of such a "contingent negative variation" (CNV), or "Expectancy-wave" (E-wave), was a *direct* function of the probability of association between stimuli.

The E-wave was then the obverse of the negative variation in the response to S_2: when this was maximal, the E-wave was minimal or absent, and *vice versa*. Thus when the probability of association between two stimuli was $p = 0.5$ or less, the E-wave was absent, but it appeared and increased in proportion as the odds "shortened" and p became greater (Black and Walter, 1965b).

Unfortunately, for those working on anterior cortical responses, the eye is an electrical dipole—and eye movements will always produce electrical variations with electrodes recording from anywhere anterior to the vertex. As a result of this mechanism, artefacts due to such eye movements always cause confusion in "DC recordings" from the anterior cortex. For some years this fact threw doubt on the validity of the E-wave as a genuine cortical phenomenon. But in 1966, Low *et al.* produced conclusive evidence of E-waves as recorded from a patient with enucleated eyes, and this aspect of the subject was cleared up.

Nevertheless both extra- and intra-ocular activities do still confuse the picture and a great deal of work remains to be done. Using high-speed cinephotography, I have demonstrated that almost complete eye-fixation can be obtained with deep-trance hypnotic subjects by simple DSUH of "you will not move your eyes". But there then remains a constant process of pupillary constriction and dilatation of intra-ocular origin and autonomic motivation. Indeed, if pupillary diameter is plotted against time in experiments where contingent habituation has been established, I have shown that the resulting curves are almost identical with those recorded from the cortex with directly coupled amplifiers, or even a long time constant.

The posterior aspect of the eye-ball is negative with reference to the anterior aspect: looking down produces a negative variation with supra-orbital electrodes, or for that matter electrodes just in front of the vertex. But this difference in potential is increased as more light falls on the retina, as may be recorded in a retinogram. If then pupillary diameter varies so precisely and synchronously with the E-wave, the question arises as to whether pupillary changes may not themselves also produce E-waves as an artefact, regardless of eye-movements, by changes in the intensity of retinal illumination.

The point has to be made here, that E-waves can easily be obtained when contingent habituation is established regardless of the nature of the paired stimuli employed—and such changes have nothing to do with the use of light, or flicker as S_2. Indeed when this is done, pupillary diameter is no longer of any value in the investigation of such phenomena. Using clicks and "clicker"—as it came to be called—as the paired stimuli, I have shown that E-waves can be obtained when the pupil is fixed with mydriatic drops in the eyes. But although this rules out any possible "retinogram effect", it can still be postulated that there is some relationship between the cortical phenomena and autonomic activity—which is not, of course, affected by local blocking at the nerve-muscle interface as with eye drops.

I now take the view that the E-wave is electroneurophysiological evidence of one form of transient memory; and that, whatever its origin, the DC shift between S_1 and S_2 in contingent habituation can provide a record of such a memory mechanism in action. Obviously, long-term memory cannot be of the same nature, and is possibly dependent on changes in the molecular form of proteins and nucleic acids, as has been suggested by Hydén and Langer (1965). And the question therefore arises, as to whether such potential changes do play any role in protein or nucleic acid synthesis. This is surely one direction in which further research should go.

But there is another approach, which is raised by the transient phenomena of hysterical and hypnotic anaesthesia and palsy. For, as suggested at the beginning of this paper, if sensation, memory, and ultimately motor function do depend on information as recorded from "improbabilities in the environment", it is conceivable that interference with the "anatomy of information", as I said, or the mechanisms of probability as in the "dilution of E-waves", might provide an explanation of these phenomena. Thus in anaesthesia, for example, far from blocking the receipt of information, hypnotic suggestion of "not feeling" may do precisely the reverse, and actually create internal hallucinatory material which renders such information "meaningless". This, then, is another direction in which my work is at present oriented.

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II. Immunological

Although brain and "mind" are all too readily identified with one another, the fact remains that, in the practice of clinical psychosomatic medicine, by far the greater part of the evidence for the influence of mind on body in creating physical disease is to be found in the field of immunology. Indeed, the development of psychosomatic medicine in the last thirty years has largely resulted from the study of allergic disease—and "allergy", as originally defined by von Pirquet (1906) with reference to the "tuberculin reaction", still remains in immunological terms "an acquired, specific capacity to react on the part of the tissues".

An "allergen" is thus an antigen which combines with antibody to produce an allergic reaction—and allergy is therefore an immunological phenomenon which derives from the activities of the reticulo-endothelial system and not from those of brain and nerves. But whether with reference to von Pirquet's original delayed type hypersensitivity (DTH) response in tuberculosis and other granulomatous diseases, or to the immediate type hypersensitivity (ITH) response of the common allergies such as asthma, hayfever and urticaria, there is now a wealth of both clinical and experimental evidence, which establishes beyond question that both these types of reaction are closely associated with the psychic state of the patient. In fact the evidence with regard to the immediate type (ITH) response is now so overwhelming, that in man, at least, ITH allergy might well be defined as "an immunological reaction dependent on the mind".

As I have pointed out elsewhere (Black, 1969), an association between mental processes and the DTH pathology of the secondary tuberculous infection appears to have been recognized for several millenia—and right from the days of Susruta to such relatively up-to-date physicians as Auenbrugger and Laennec, writers have tended to attribute this easily recognizable "wasting disease" to the emotions of frustated love. In this century is was Wittkower (1952) who described for the first time the "inordinate need for affection" of his tuberculous patients in aftercare, and Kissen, inspired by this finding, entered upon his three famous investigations on *Emotional Factors in Pulmonary Tuberculosis* (1958).

Kissen's well controlled clinical studies, in which the psychiatric analysis of personality was correlated with X-ray diagnosis, showed in three separate series, that a tuberculous infection of clinical proportions is statistically "most likely"—i.e. in more than 60 per cent of cases—to follow some "break in a love link", whether due to a broken love affair or a broken marriage, a break between parents and children, or even a rift between siblings. At the conclusion of his work in this field, Kissen was so sure of his ground that, in the presence of the ubiquitous tubercle bacillus of the slums in Glasgow, he was able to predict with accuracy which of his initially uninfected controls would eventually succumb to the disease.

Working with HUMPHREY, Head of the Immunological Division of the Medical Research Council at the National Institute for Medical Research at Mill Hill, London and with JANET NIVEN, who was Head of the Cytological Division, I was then able to show that the Mantoux reaction, as elicited with precision through the intradermal inoculation of PPD, can be inhibited *clinically* by direct suggestion under hypnosis (DSUH) in suitable deep-trance hypnotic subjects. In these cases, full-thickness skin biopsies, taken 48 h after inoculation, showed that although the initial lymphocytic infiltration of the normal, uninhibited response in Mantoux-positive individuals was still present, in the psychologically inhibited response there was nevertheless failure of the reaction to proceed to the usual exudation of fluid into the tissue space. And it was essentially this absence of fluid exudation which eliminated the normal "firm, raised swelling" of positive clinical diagnosis, and produced "Mantoux-negative" results in Mantoux-positive individuals.

As we pointed out in our paper (BLACK, HUMPHREY and NIVEN, 1963), there are two elements involved in the skin response of the Mantoux reaction: the manifestations of the DTH allergic reaction which includes lymphocytic infiltration, and the manifestations of an ITH reaction characterized by the release of histamine and possibly serotonin. And it is, of course, this second element which accounts for the subsequent failure of the skin vessels to contain the blood fluids.

Our work showed, therefore, that whatever the effect of DSUH "not to react" on the DTH element in the Mantoux response, it was basically inhibition of the ITH element which resulted in a "Mantoux-negative" diagnosis following such psychiatric treatment. Indeed, so far as could be observed, no effect at all was produced on the DTH element.

Now I had already proved fairly conclusively that the ITH response can be almost "instantly" inhibited by DSUH in suitable subjects (BLACK, 1963a), and that even the Prausnitz-Küstner reaction, resulting from passive transfer of skin-sensitizing serum, can also be inhibited in this way (BLACK, 1963b). By the passive transfer of serum from individuals in whom psychic inhibition had already been produced, I had also shown that no change in the level of serum antibodies occurred in the course of these phenomena (MASON and BLACK, 1958).

The question arose, therefore, as to the nature of the physical mechanisms involved—if not the nature of "mind" itself—and the experiments reported here were originally designed to investigate this problem. Since the reticulo-endothelial system is notoriously devoid of any direct motivation by the central nervous system, one likely hypothesis concerned the activities of the hypothalamic-pituitary-adrenal axis and the release of 11-hydroxycorticoids. These endogenous steroid hormones are essentially three in number, and in man they are generally assayed together in terms of "cortisol". Although evidence as to the effects of such hormones on allergic reactions was lacking, since the ITH response *can* be inhibited by therapeutic doses of hydrocortisone, it was reasonable to suggest that, via the hypothalamus, endogenous production of cortisol might produce the same effect as that following DSUH of "you will not react".

Inhibition of the Prausnitz-Küstner reaction by DSUH is usefully investigated by producing a shift in the dose-response curve which results when graded dilutions of the donor's serum are inoculated into a series of sites down the forearm. If measurements are then made of the areas of the weals resulting when the allergen to which the donor is sensitive is pricked in, serum dilution can be plotted against the logarithm of weal area—and a straight line results. Inhibition of these responses by DSUH then produces a shift in this dose-response curve, and the degree of inhibition is a measure of the degree of shift. My work on this phenomenon (BLACK, 1963b) had already established that the degree of inhibition was an inverse function of the concentration of the donor's serum, and a direct function of the depth of trance into which the subject could be hypnotized.

Working with FRIEDMAN, I therefore investigated the role of adrenal function during the production of such a shift in the dose-response curve of the P-K reaction by DSUH. First, we estimated the plasma cortisol levels during the production of a shift in the dose-response curve by DSUH; and secondly, we estimated the shift in the dose-response curve during an artificial rise in the plasma cortisol levels produced by adrenal stimulation with adrenocorticotrophic hormone (ACTH)—which was given in dosage established as being within psychophysiological limits.

We were not concerned in this instance to provide further evidence that DSUH does inhibit the ITH response—or a statistically significant series in support of our findings. Assuming the inhibition by DSUH of the ITH response as a fact, the object then was to determine whether adrenal function played any kind of a role in this phenomenon. Unless a preliminary finding showed that adrenal function *did* play a role, it would be unnecessary to experiment on more than one subject. In fact we performed 12 experiments on 2 subjects—both female.

The plasma cortisol was estimated by a modification of the fluorimetric method first described by MATTINGLY (1962). There is a normal diurnal rhythm in plasma-cortisol levels, which are maximal between 08.00 and 09.00 h and lowest in the early hours of the morning (MATTINGLY, 1963). Thus the level falls steadily throughout the day in people of good health, and we therefore conducted our experiments over a standard time of day, between 14.00 and 16.00 h. Blood samples were taken from an in-dwelling intravenous catheter (to avoid the stresses involved in the pain of venepuncture), and after a baseline was established from 14.00 to 15.00 h, a shift in the dose-response curve of the P-K reaction by DSUH was demonstrated in two sessions, at 14 days' interval.

Thus, twenty-four hours before each session, at 15.00 h, graded dilutions of an anti-horse human serum were intradermally inoculated at five sites on the flexor aspect of the forearm. On the day of the experiment, the subject rested in a couch and the intravenous catheter was introduced. Blood samples were taken at 14.00 h, and then at intervals of 30, 60, 75, 90 and 105 min. Following the 60-min sample, at 15.00 h, drops of normal horse serum were placed on the inoculation sites. These were pricked in and the horse serum left *in situ* for one minute, and the resulting weal areas were recorded after 15 min on adhesive, transparent tape and measured later by planimetry. Fourteen days afterwards the whole procedure was repeated on the other arm, but following DSUH given with the words: "there will be no reaction".

As expected, there was clear-cut evidence in both subjects of a substantial shift in the dose-response curve following such DSUH "not to react". In Subject I there was complete inhibition of the normal response at a dilution of only 1/10; and in Subject II, inhibition was from 32 to 17 sq. mm. at 1/10, and complete at all other dilutions up to 1/1000. But following DSUH "not to react", there was no indication in either subject of any change in the normal diurnal rhythm of the plasma-cortisol levels and, as under normal, control conditions, these continued to fall steadily over the following 60 min.

The results of these experiments seemed to prove fairly conclusively that a rise in the cortisol levels is not involved in the mechanisms of inhibition of the ITH response by DSUH. But there remained the possibility that a *physiological rise* in the plasma-cortisol levels might still inhibit the allergic reaction. And the problem then was to ascertain what kind of cortisol levels might be expected as the result of psychophysiological mechanisms operating via the hypothalamic-pituitary-adrenal axis. Our aim, therefore, was to assay the physiological levels of the plasma cortisol as these might occur following DSUH "not to react".

To carry out this assay, the basic problem was to produce the appropriate emotional state in the subject under controlled experimental conditions—and to estimate the cortisol levels throughout the period of emotion. A baseline was therefore recorded in the usual way for the first 60 min of the experimental period, following which forceful DSUH of "being frightened" was given over 45 min to elicit the maximum subjective reaction possible.

The words used were: "You are frightened . . . You feel frightened . . . You are frightened of something . . . You are frightened . . .". As expected, the subjects responded to such suggestion with increased heart rate, increased respiration rate, sweating hands, and often a considerable histrionic show of weeping and facial contortion. Compared with the normal diurnal rhythm, this DSUH of "fear" also produced an immediate rise in the cortisol levels: from a mean of about 14 micrograms per 100 ml. of plasma, to nearly 30 micrograms. An immediate drop then followed, when the suggestions were "cleared" and a happy, tranquil state was created by further suggestion before waking.

In effect, DSUH of "fear" had approximately doubled the blood-cortisol levels. It therefore remained to titrate the subjects with ACTH in an intravenous drip, so as to estimate the quantity of ACTH required to produce variations in the plasma-cortisol levels of the same order. The answer in both subjects was found to be 9 international units (IU) of crystalline ACTH over 45 min in a dextrose-saline medium. Everything was now ready for the second series of experiments.

To carry out these experiments, the dose-response curve of the P-K reaction was first elicited in the waking state on one arm in the usual way—but in the presence of an intravenous drip of dextrose-saline, so that in both parts of the experiment the all round physiological conditions would be identical. This process was then repeated at a later date on the other arm—with the difference that the solution was altered, so that the drip bottle delivered the required 9 IU of ACTH over the next 45 min after the last resting blood sample had been taken. The answer then turned on the evidence of any shift, or not, in the dose-response curve as the result of such ACTH administration. The experiment was carried out on both subjects.

The results showed quite conclusively that, in spite of rises in the plasma-cortisol levels of the same order as those produced by DSUH of "fear", there was no shift in the dose-response curve in either subject. It was therefore concluded on very substantial evidence indeed, that the shift in the dose-response curve of the P-K reaction which can be produced by DSUH "not to react" is *not* due to any rise in the plasma-cortisol levels as the result of hypothalamic-pituitary-adrenal activity. From which it could be argued with some force that any rise in the plasma-cortisol levels is extremely unlikely in the inhibition by DSUH of the ITH allergic reaction in general.

The "missing link" between the central nervous system and the immunological phenomena of psychosomatic medicine is not therefore likely to be found in the secretion of steroids by the suprarenal cortex. But it cannot be said that my further work since the publication of our paper on these experiments (BLACK and FRIEDMAN, 1965) has thrown any more light on this difficult subject.

The ITH response is clearly amenable to psychological control: the effect is virtually "instant"—it happens within a few minutes at most; it is relatively permanent, as evidenced by clinical findings; and interestingly enough, it cannot readily be reversed by further DSUH. These seem to be the facts; and so long as we adhere resolutely to a theory of mind as manifested in the electrical mechanisms of the central nervous system, and only in the central nervous system, it is difficult to see on present evidence how any explanation could be found. It is possible, of course, that there are undiscovered hormones from the pituitary itself which are released through stimulation of the hypothalamus and produce these inhibitory effects on the allergic reaction; but this is pure conjecture and must remain so until such hormones are isolated from the blood plasma.

In considering the effects of DSUH we are involved with the transfer of information, and as in the study of neurological responses, the semantics of the hypnotic instruction become important. FRIEDMAN and I have shown, for example, that the modality of emotion suggested plays an important part in the release of cortisol—which is increased in the blood plasma in the presence of fear and anxiety, but is not so increased in "happiness", or any associated state of euphoria (BLACK and FRIEDMAN, 1968).

Clearly fear and anxiety are "stressful", and presumably happiness and the associated euphorias are not. But it was a significant additional finding of our work that, although pain produces a rise in the plasma-cortisol levels, the same painful stimulus in the presence of hypnotic anaesthesia does not do so.

Thus, whatever the informational mechanisms implied by the E-wave, discussed in the first of these two papers, the effect of DSUH with the words "you will feel no pain" is certainly in advance of the input to the hypothalamus by which pituitary stimulation is achieved through the pituitary portal system. Unless, that is to say, we are prepared to postulate yet another hormone which neutralizes either the effect of ACTH, or cortisol itself.

Phylogenetically the central nervous system is relatively new, and the "primaeval informational problem" of the primaeval organism must have been the basic recognition of the material substance of "self" from the material substance of "non-self" in the environment. The complex informational systems of immunology are therefore phylogenetically very old, and in considering the evolutionary history of what we like to call "mind" it seems reasonable to suppose that this is how it all began.

Thus an antigen is essentially a cybernetic "source", which evokes through physical contact with the "receiver" a response recorded as antibody. And this in turn becomes the physical substance of one form of "memory". From such mechanisms of physical contact, the highly specialized central nervous system must have evolved; and if the experimental facts of psychophysiology indicate an intimate connection between the two, this is all to be expected, even if the nature of the connection is still obscure.

Perhaps the fundamental error here lies in the general acceptance of "man-made wiring" as the biological model on which almost any discussion of brain and nerves is based. Looked at in another way, nerve cells become simply "very long cells", and otherwise subserve their functions according to the same principles as any other cells. And these principles, as we know, are largely based on physical contact between the cell and the environment—or the molecular form of the environmental substance.

We already know, for example, that in the primaeval sense of smell—to which even in man a huge proportion of the brain substance is still dedicated—the basic olfactory mechanism of the sensory end-organ is a recognition of molecular form by physical contact, or an interpretation of the steric effects of complex molecules (AMOORE *et al.*, 1967). And on the evidence of psychophysiological research, it might be supposed that a similar mechanism of steric dependence is involved at the ultimate sensory nerve ending in the presence of antigen.

In these terms, the olfactory function becomes a specialized version of a general bodily function—and in effect, we "smell" the presence of antigen no less than the presence of a foreign substance in the nose. By "interference with the informational systems of neurology", as outlined in the first paper, the olfactory sense can easily be eliminated by DSUH of "you will not be able to smell anything". Perhaps, therefore, the words "you will not react" produce a similar effect on the immunological response? The lymphocytes may otherwise convey the information to the reticulo-endothelial system with a consequent production of antibody—as seems to be the case. But even in the presence of antibody, as I have shown (BLACK, 1958), something is then missing following DSUH of "you will not react", and the allergic immediate type hypersensitivity response is not evoked. Further research in this field should surely be aimed in this direction.

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A Study on Hypnosis in the Rabbit

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More or less prolonged states of immobilization have been described in several animal species. These are produced by various external factors, such as a forced abnormal posture (supine position), or particular patterns of sensory stimulation, e.g., low-frequency, monotonous repetition of visual, auditory or vestibular stimuli. Various authors writing about different species have called this phenomenon akinesia, catalepsy, catatonia, cataplexy, tonic immobilization, hypnotic state, hypnosis, etc.

Long before electrophysiological techniques were introduced, various interpretations had been suggested for this kind of phenomenon (see Chertok, 1968). One of the most interesting is Pavlov's (1923) suggestion, that hypnosis is an intermediate state between wakefulness and sleep, during which cortical activity is "partially" inhibited, although a certain degree of alertness persists. A more recent version of this theory is that of Svorad (1956, 1957), who attributes a role to the "reticular formation", hypnosis being thus considered as the result of an inhibitory process at subcortical levels.

Electrophysiological studies on animal hypnosis have so far been concerned mainly with neocortical or rhinencephalic "spontaneous" electrical activity. In brief, they all indicate that there are no specific electrical symptoms of the hypnotic state. Hypnosis is not *necessarily* accompanied by states of drowsiness or sleep, although immobilization can favour a decrease of alertness. In fact, these investigations of bioelectrical phenomena have not yet led to any really new hypotheses on the mechanisms involved in hypnotic states.

The data to be presented in this paper were obtained from the rabbit. In this species, as is well known, a stereotyped escape or "flight" reaction, consisting of synchronous, bilateral flexion and extension of the hind legs, can be readily produced. Moreover, it is known that such stereotyped locomotor patterns can be inhibited under various circumstances, thus immobilizing the animal. The most spectacular procedure is to force the subject into a supine position. Another method of immobilization consists in submitting the animal to a repetitive, monotonous, diffuse light stimulation (flicker) at a low frequency.

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* Part of the experimental work described in this paper was carried out in collaboration with Drs. L. Chertok and M. Fontaine.

Our observations concern various features of this "induced akinetic state". They are mainly based upon the fact that, in "acute" preparations (see Methods) with only weak anaesthesia, and paralyzed with a curarizing drug, rhythmic discharges, which are equivalent to locomotor movements, can be recorded from hind limb motor nerves. In addition, such discharges can be inhibited by various external factors, including precisely those which produce akinesia in a normal animal (VIALA and BUSER, 1965; BUSER *et al.*, 1967; BUSER and VIALA, 1968).

The experimental conditions thus fulfilled provide better possibilities for analytical investigations than would a study exclusively performed in normal animals, carrying implanted electrodes.

Methods

The experiments were carried out on various types of rabbit preparations: 1) animals first deeply anaesthetized with nembutal, then later paralyzed with flaxedil, put under artificial respiration and left with only light narcosis; 2) decerebrate animals (the operation being performed under ether); 3) free animals with implanted electrodes. In "acute", non-transected animals, recordings from various neocortical loci were made with silver macroelectrodes. Recording from the reticular formation was performed with metal (tungsten or steel), insulated semi-microelectrodes (5—10 μ in diameter, insulation up to the tip). During deep anaesthesia, hind-limb nerves were prepared—either the central stump of the sciatic nerve alone, or more frequently, one branch to a flexor (*tibialis anterior*) and one to an extensor (*gastrocnemius medialis*). Depending on the particular needs, the animal was either left without head fixation, or placed in an adequate stereotaxic holder (CHATELIER and BUSER, 1961), for brain stem microelectrode recordings.

Somatic stimulations consisted most often of electric shocks delivered to one or the other leg, through needles introduced subcutaneously into their dorsal aspect. In some particular cases, weaker cutaneous stimuli had to be given; they were obtained by blowing air puffs of given pressure and duration on a small skin area (3 to 10 sq. cm) at regular intervals. Short, repetitive light flashes were delivered at variable frequencies (1 to 50/sec) The stroboscopic gas tube was located at about 20 cm in front of the animal, thus providing a diffuse, bright illumination of both eyes.

"Chronic" implanted animals were seldom used, and then only as controls. They bore only cortical electrodes, consisting of silver balls maintained by dental cement.

On several occasions, semi-quantitative evaluations of the discharges were undertaken. That is, nerve discharges were applied to an electronic device converting frequencies into voltage, thus providing an analogue curve, whose amplitude above base line is at each moment proportional to the impulse frequency. On such records, the number of peaks per unit time (1, 5, 10 or 60 sec, depending on the particular case) could be easily measured, providing an evaluation of the "frequency" of the burst over unit time. In other cases, when the duration of each burst also varied, a more elaborate quantification was performed. A planimetric measurement of the surface occupied by the bursts was made on a standard record. The number thus obtained (*discharge index* for a given time) gives a more accurate picture of the total nerve activity, not taking into account, however, the frequency of successive bursts.

Results

A. Escape movements in the decerebrate preparation; their inhibition through pressure on the lumbar skin area. Decerebrate rabbit preparations frequently show, instead of a permanent extensor hypertonus, rhythmic movements of flexion and extension of both posterior limbs, which closely resemble the flight pattern in normal animals. These stereo-

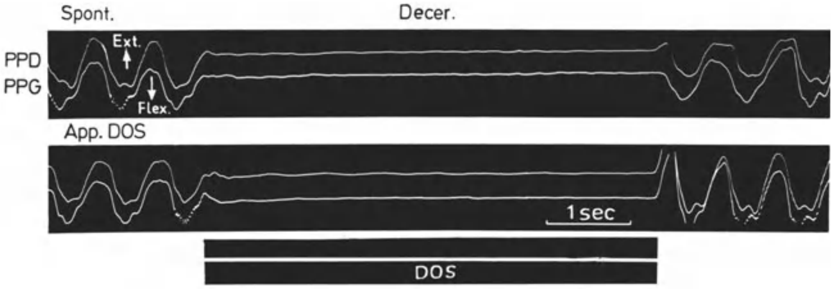

Fig. 1. Decerebrate rabbit. Simultaneous strain-gauge recording of "spontaneous" rhythmic movements of both hind limbs (PPD, right; PPG, left). Upward deflections correspond to extension and downward deflections to flexion. In upper record, movements underwent a "spontaneous" interruption during a few sec; in lower record, an interruption of similar duration was produced by manual pressure on the back (as indicated by the broad line, "Dos"). Note that a post-inhibitory rebound is present in latter case only after cessation of dorsal pressure

typed locomotor patterns appear either spontaneously or following one or another kind of somatic stimulation. Such movements can be completely abolished when a slight pressure is exerted (for example by hand) upon the dorsolumbar skin area. The suppression lasts as long as the pressure is maintained, and the first movement after cessation of pressure shows a typical postinhibitory rebound (Fig. 1).

This observation was our first indication that an inhibition of the automatic locomotor movements in the rabbit, and therefore an immobilization of the animal, could be caused by a natural stimulation consisting of a slight pressure on the dorsal cutaneous area. The same pressure also occurs when the animal is forced to adopt a supine position.

We then performed experiments on paralyzed preparations, only recording discharges from dissected nerves.

B. Rhythmic efferent discharges; their suppression through dorsal skin pressure. In preparations under light anaesthesia, paralyzed with curare and intact except for the dissection of motor nerves (see Methods), rhythmic discharges can be recorded from the central stump of the sciatic, or from nerves to a flexor and to an extensor. When recording is made from the two antagonistic nerves, discharges show perfect alternation (Fig. 2). Again, depending on the state of the preparation, they develop either

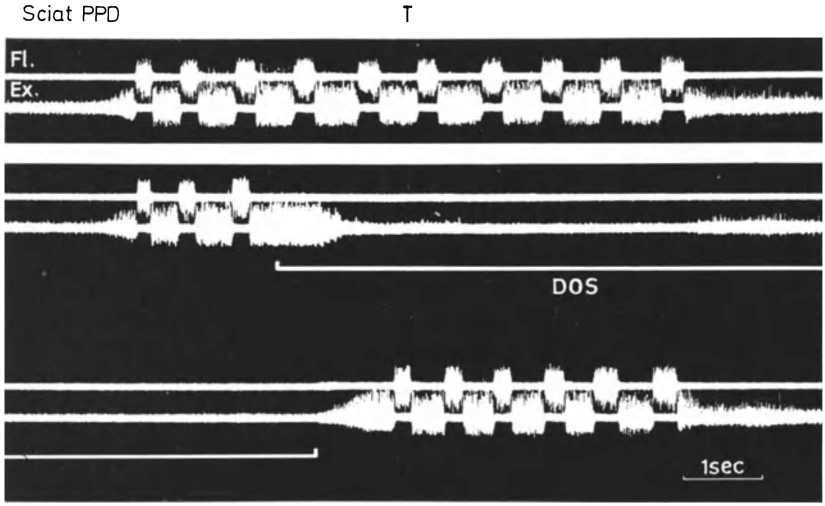

Fig. 2. Curarized intact preparation. Nerve activities were simultaneously recorded from one nerve branch to a flexor (Fl) and from another to an extensor (Ex) (see text). Upper record: spontaneous rhythmic discharges corresponding to locomotor movements. Lower record: effect of hand pressure on the dorsum of the preparation ("Dos"). Note suppression of rhythmicity

spontaneously or following somatic stimulation (air puff on nose, or single or repetitive electric shocks to one or the other leg). Because of their frequency (1—3 per sec) they have to be considered as motor discharges corresponding to rhythmic movements of locomotion. Rhythmicity is thus "programmed" in the central nervous system, the proprioceptive sensory feedback (which is suppressed by curare) not being essential to their development.

In such preparations, spontaneous and elicited discharges are again abolished while pressure is exerted on the lumbar skin area (Fig. 2). In general, discharges in the extensor nerve are somewhat less affected than those in the flexor nerve; it is likely that this particular aspect of the inhibitory action explains why, during akinesia induced by the supine position, the animals' limbs are in relative extension.

The lumbar stimulation is apparently not nociceptive, as far as this can be appreciated subjectively. Stimuli considered as painful are, on the contrary, excitatory. Moreover, no such inhibition has been obtained through electrical stimulation of cutaneous nerve endings or cutaneous nerves at the lumbar level. Finally, it has not been possible to determine the relative role played by exteroceptive (i.e. cutaneous), as compared with proprioceptive (lumbar muscles, tendons, aponeurosis, etc.), components of sensory excitation. This inhibitory action is in all likelihood dependent upon a given spatio-temporal pattern of afferent signals originating from a given body area.

Inhibition could also be obtained from other parts of the body, e.g. the abdomen or, in certain animals, the dorsal aspect of the legs. However, the threshold for inhibition was, as a rule, much lower in the dorso-lumbar area than anywhere else. In addition, most of the time, areas such as legs, face, ears or nose, appeared to be mainly excitatory, whatever the type of somatic stimulus applied.

C. Participation of supraspinal structures: role of the bulbo-ponto-mesencephalic reticular formation. Several of our data indicate that the reticular nuclei in the medulla and the pons are essential for organizing the rhythmic locomotor movements, and also for their inhibition through the dorsal pressure manoeuvre.

Thus, rhythmic discharges persist practically unchanged after removal of the telencephalon (neocortex and rhinencephalon) and the diencephalon. As was mentioned above, they exist in decerebrate animals. On the other hand, it has not yet been possible to obtain them either after acute transection of the spinal cord, or in chronic spinal animals.

Moreover, single unit recordings from various points in the medullary, pontine or mesencephalic reticular formation have revealed neuronal activities which show significant increases in their discharge pattern, immediately preceding each spontaneous efferent burst.

This precedence of the central activation relative to the peripheral one (when no external stimulus is applied) is suggestive of a causal relationship. As a first approximation, one is thus inclined to consider that the pacemaker for rhythmic locomotor stereotyped discharges is located in the bulbo-ponto-mesencephalic reticular structures, with reticulospinal neurons transmitting rhythmic excitation to the spinal efferent cells. Similarly, the "pacemaker" units thus identified in the medulla, pons and mesencephalon, are inhibited during dorsal pressure, again with some anticipation with regard to the peripheral blockade (Fig. 3). It is thus likely that stereotyped pacing is not only related to the medulla for its organization, but also for its inhibition. This would mean that "hypnotic immobilization" is a process taking place at the supraspinal reticular levels.

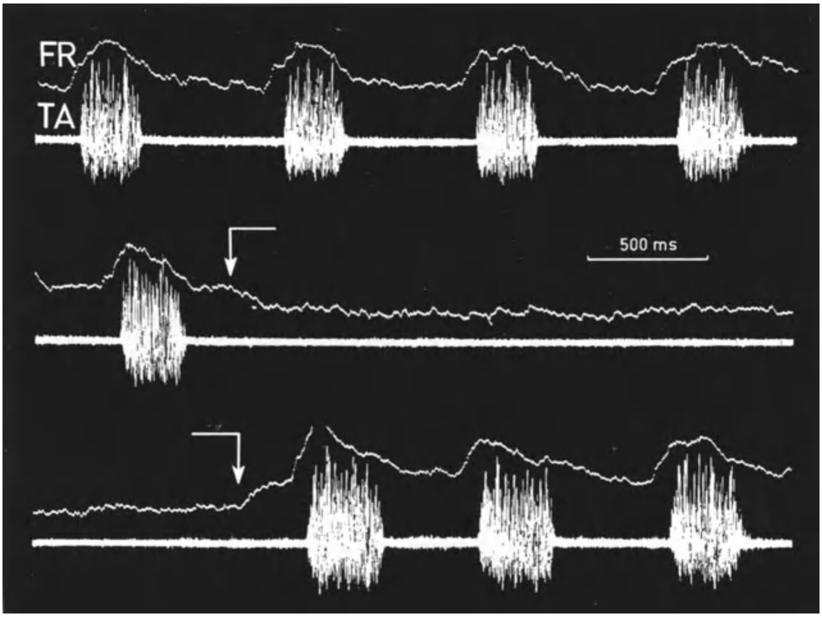

Fig. 3. Simultaneous recording of activities from the mesencephalic reticular formation (FR) and from the nerve to *tibialis anterior* (TA). Reticular discharges were fed into the frequency-to-voltage converter (see text). *First row:* each "spontaneous" burst in the nerve is preceded by an increase in reticular activity (upward deflection). *Second and third row:* manual pressure on the back of the preparation (between arrows) suppresses nerve activity and, at the same time, causes decrease of the reticular activity. Note postinhibitory rebound at both recording sites

D. Reflex immobilization and electrocortical activity. In free animals with implanted electrodes, we could once more confirm that hypnosis (through forced supine posture) is not accompanied by any particular electrocortical pattern. Depending on the subject and on the experimental circumstances, the immobilized animal either remains alert (with an aroused electrocorticogram), or falls into drowsiness and even into sleep, with slow cortical waves or spindles (Fig. 4). The latter situation is reversible, as various stimuli may arouse the animal, although without suppression of the hypnotic postural state. Once more, the dissociation between sleep and the akinetic state is thus clearly confirmed.

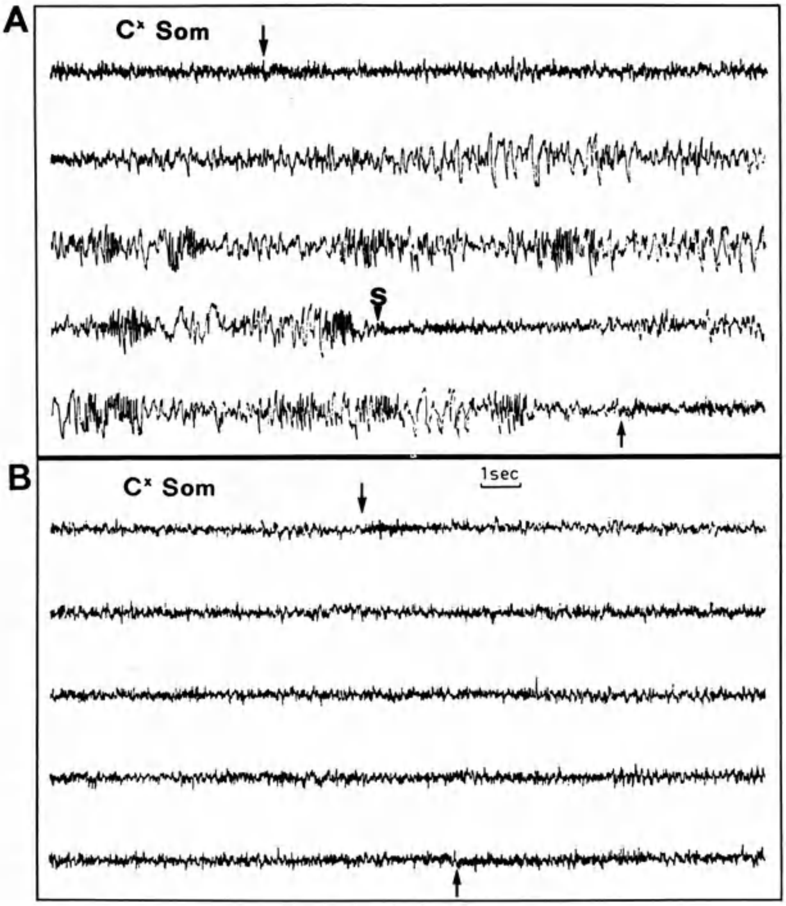

Fig. 4. Electrocortical activity on the sensorimotor cortex during hypnosis, in two different animals (A & B). Single continuous recording. In each case, akinesia produced by forced supine posture ("hypnotic state") took place between arrows. During hypnosis, animal A displayed electrocortical patterns of drowsiness (slow waves and spindles); at S, a sound aroused it without interrupting the hypnotic posture. Animal B remained awake throughout the period of akinesia

On the other hand, in curarized preparations, in which "flight" and "immobilization" are tested by recording efferent nerve activities, the dorsal pressure almost always induces a pattern of drowsiness (Fig. 5); this occurs about 3 to 4 sec. after applying the pressure stimulus, somewhat later than the peripheral inhibition. Similar observations on the development of slow electrocortical patterns were made earlier by TAKAGI *et al.* (1959) in man, and KUMAZAWA (1963) in the rabbit.

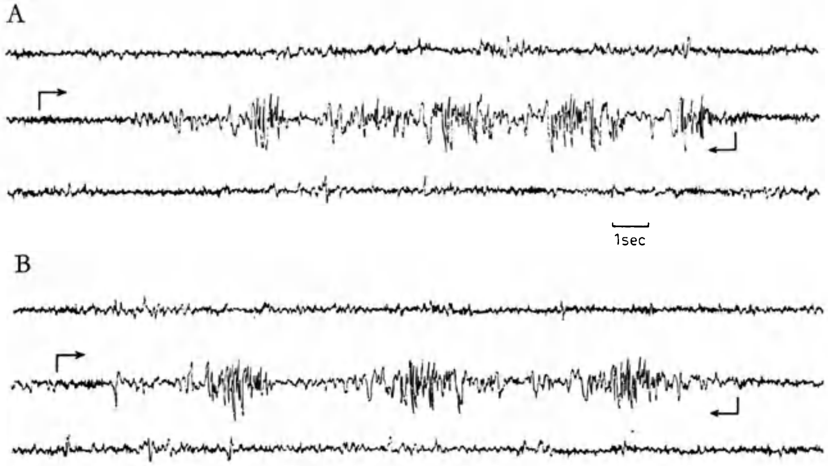

Fig. 5. Electrocortical recordings from sensorimotor cortex (single channel), in two distinct cases: "acute", curarized preparation (xylo-flax) (A), and free animal with implanted electrodes (B). In both cases, pressure on the back (between arrows) induced slow electrocortical patterns, suggestive of "drowsiness"

E. Immobilization through repetitive light flicker. In paralyzed, slightly anaesthetized, but non-transected preparations, it has also been possible to observe a suppression of the peripheral discharges during application of a repetitive light flicker. The experimental conditions were similar, discharges being either spontaneous or elicited through somatic stimulation.

In general, the spontaneous activity during flicker was reduced to a level of 20 to 40% of the control value. Some experiments were performed after I.V. administration of amphetamine, taking advantage of recent findings that "spontaneous" rhythmicity is enhanced by this treatment (VIALA and BUSER; unpublished). In such cases, the inhibitory effect of intermittent light stimulation was particularly clear (Fig. 6 and 7).

Elicited bursts were, in this instance, produced by blowing air. Each puff (delivered every five seconds) elicited one burst. Under such conditions, the inhibitory effect (i.e. "immobilization") of light showed up very clearly. On the contrary, it appeared that discharges elicited by a strong stimulus, e.g. subcutaneous electric shocks, were not inhibited by light.

In all cases, the optimal frequency for inhibition was about 3 flashes per sec. Because habituation rapidly occurred, the flash sequences were not given at less than 30-minute intervals. After suppression by light, a rebound could sometimes be observed (a fact which, again, indicates that nervous inhibition actually occurred).

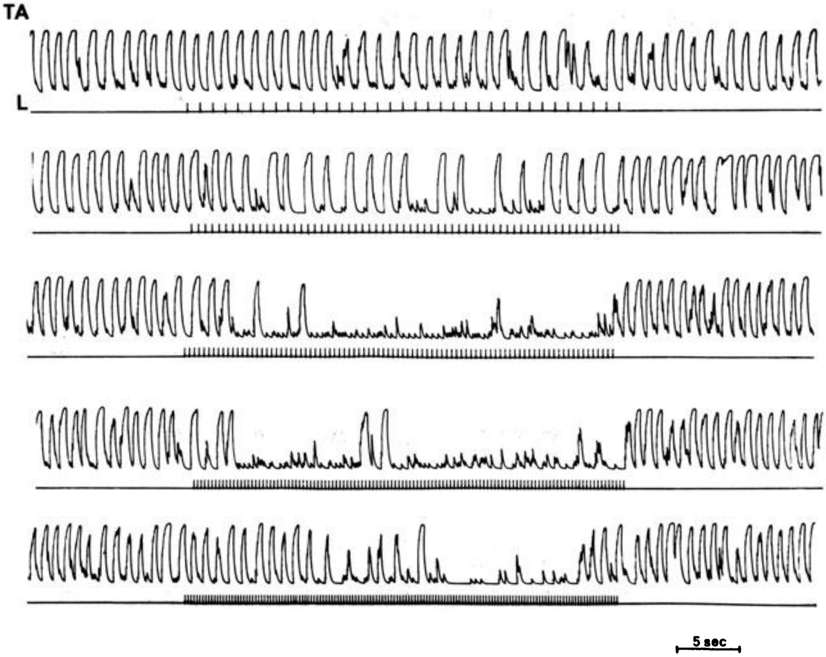

Fig. 6. Inhibition of spontaneous rhythmic locomotor discharges in a flexor nerve through light flicker at various frequencies (photo-electric artefact indicated on bottom channel of each group of records, L). Recorded discharges were fed into a frequency-to-voltage converting device (see text). Each peak represents a burst of discharges in the flexor nerve. Flicker frequencies are, from top to bottom, 1, 2, 3, 4, and 5/sec. Note optimal inhibitory effect at 3 and 4/sec. A 30-min interval was left between two successive trials. Curarized preparation

Concluding Remarks

Conclusions reached are as follows:

1. In the rabbit, a natural stimulation (pressure) on a given skin area, namely the dorso-lumbar zone, can induce inhibition of the rhythmic flexion and extension representing the stereotyped locomotor pattern in this species.

2. This fact provides a simple explanation of the well-known akinetic immobilization which occurs when a rabbit is forced into a supine posture. The first determinant of akinesia is the inhibitory effect of this dorsal pressure maintained by the weight of the body.

3. Immobilization maintains pressure, which in turn maintains immobilization; a positive feed-back mechanism thus occurs, which facilitates the persistence of the akinetic state.

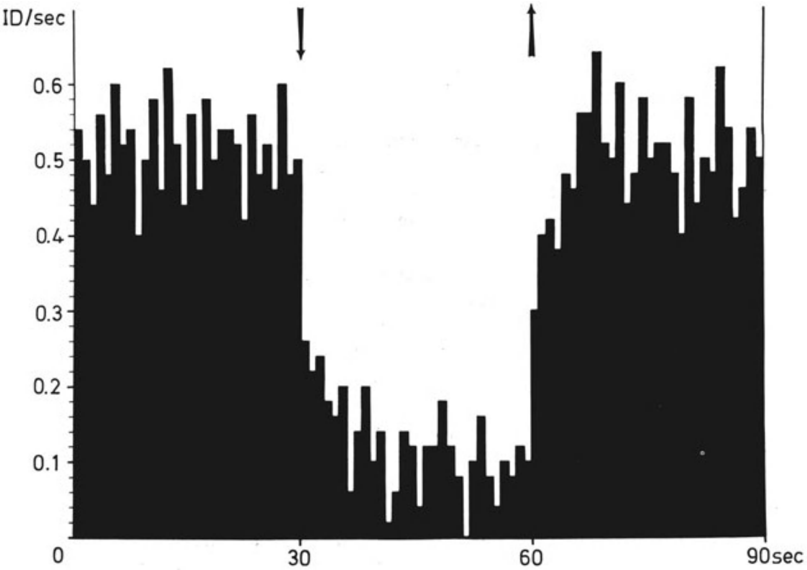

Fig. 7. Inhibition of rhythmic spontaneous flexor discharges through flicker. Light flashes were delivered during 30 sec, at 3/sec, (between arrows). In this experiment, nerve activities were evaluated by measuring "*Discharge index*" per sec (ID/sec). Notice that inhibition occurred almost immediately and lasted through end of visual stimulation. No post-inhibitory rebound occurred in this case

4. Postural immobilization may, or may not, lead to drowsiness and sleep; a decrease of alertness is not necessary to the process of immobilization: drowsiness may simply serve to reinforce the positive feed-back loop as described above.

5. It is likely, from our data, that the "programming" of the stereotyped locomotor activity is located at supraspinal levels of the reticular formation. It is probable that the inhibition of those movements also takes place at the same level.

6. Other factors—or particular patterns of sensory stimulation—may also induce immobilization (this time, without important changes in posture). This is especially the case with repetitive and monotonous stimuli (Pom-

PEIANO and SWETT, 1962; PEYRETHON and DUSAN-PEYRETHON, 1967), or with diffuse and constant (non-patterned) visual or acoustic stimuli (BALASSA, 1828; BEARD, 1881; CZERMAK, 1872, 1873; HABERLAND, 1926; LÉPINAY, 1903; OCHOROWICZ, 1909; SVORAD, 1957; PEYRETHON and DUSAN-PEYRETHON, 1967; VÖLGYESI, 1938). In our experimental series, we could observe inhibition of efferent rhythmic motor discharges through repetitive, diffuse flicker at 3/sec. The pathways involved in this other type of induction have not yet been identified.

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Hypnosis and Pre-sleep Patterns

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I should like to consider one particular neurophysiological aspect of hypnosis—that of electroencephalography. The basic question is to ascertain whether there exists some relation between hypnosis and sleep, or pre-sleep patterns. The great majority of investigations on sleep have so far been focussed essentially on the study of two phases of sleep: that associated with slow EEG activity, on one hand; and that with fast EEG activity, or stage 1-REM sleep, on the other. Very few investigators, however, have directed their attention to the EEG patterns in drowsiness, and it is this particular aspect that I propose to discuss here. Almost all authors are agreed that, amongst the phases of sleep, it is possible to distinguish: stage 1 (known in America as "1—non-REM"), which is a stage of light sleep; the stages of slow wave sleep, which would include stages 2, 3 and 4; and finally, the stage of dreaming sleep, which is called "1-REM" in the USA, and "PMO" (phase of ocular motion) in France. These, then, are the constituent parts of sleep: drowsiness, slow wave sleep, and dreaming sleep.

It is my personal opinion that the polygraphic aspects which characterize drowsiness are not well known, and I should, therefore, like to present here some of my own views with regard to phase 1 sleep. I subdivide this phase into three stages: 1a1, 1a2, and 1b. It is not very easy to study this phase, but one of the methods which I have found useful for this purpose is to ask a normal subject to read a book when he is very tired—for example, when he has been wholly or partly sleep-deprived for one or two days. The subject is very sleepy, and I ask him, let us say at 9 p.m., to read a book in bed and to continue reading as long as possible, until he gradually falls asleep. The EEG is recorded from different areas (e.g. central and occipital—bilateral). Recordings are also made of vertical eye movements so that blinking artifacts may be detected and of horizontal eye movements in order to obtain what I call the "scopogram", which indicates a particular way of reading. In addition, we record the electromyogram of muscles showing a continuous tonic activity during wakefulness (e.g. the cervical or the mylohyoid muscles), the electrocardiogram, etc.

When the subject is reading, the scopogram shows large deflections when he is passing from one line to the next, and frequent small deflections in the opposite direction, which represent the jerky eye movements occurring when he is reading the words on a single line.

At first, when the subject is reading, there is no alpha activity. Suddenly, after five to twenty minutes, but generally quite soon, the scopogram shows some large movements and no more jerky movements; at this stage the subject is no longer reading. Then there is a lot of blinking and, just at the moment when the subject stops reading, waves in the alpha range appear. The distribution of this alpha activity is somewhat more diffuse than that of the alpha rhythm recorded in the same subject while he is awake with the eyes closed. Very soon afterwards, generally within five or ten seconds, there is a kind of fragmentation of the alpha activity and some more blinking, and then the blinking ceases. Finally, all the alpha waves disappear and are replaced by a low voltage theta-like activity. The EMG shows a considerable diminution of tonic muscular activity. In these EEG sequences, I distinguish as stage "1a1" that of the appearance of the alpha rhythm; as stage "1a2" that of the fragmentation of the alpha activity; and as stage "1b" that of the appearance of waves in the theta range.

It is very difficult to determine whether phase 1 sleep is, in actual fact, drowsiness or very light sleep. In my own mind, however, there is no doubt at all that this is no longer drowsiness but true (though very light) sleep. Indeed, there very soon appear the first transient phenomena typical of sleep, i.e. some sharp waves recorded at the vertex and some bursts of spindles characteristic of stage 2. For this reason I believe that phase 1 is simply a transition to stage 2, and is already true sleep. Generally at this stage one observes that the subject has stopped reading and has let the book fall, and I agree that at this moment he may experience some degree of hypnagogic thinking (while showing little reaction to reality). Sometimes at this stage, instead of hypnagogic hallucinations, there may be some dreaming. Personally, I call this dreaming. I am aware that a true hypnologist would not agree that dreaming may be present at this stage and would say: "Dreams occur only in the 1-REM stage". For my part, I am absolutely certain that, in this phase, one may encounter something which it is impossible to differentiate from the dream, except that it does not appear during the "dream phase" of sleep.

Obviously, dreams often occur in another EEG phase, the 1-REM, but some dreaming can also take place in the phase under discussion. For this reason, I make a distinction between hypnagogic dream and hypnagogic reverie. I believe that in cases of this kind it is possible for both of these states to coexist. It is impossible to differentiate hypnagogic dream from true dreaming.

Now the problem is to ascertain whether there is any relationship between these EEG patterns and those observed during hypnosis. I do not believe that there is any easy answer to this question, and I have not personally studied hypnosis from this point of view. I am, however, at the present time going through the literature on this subject. In this process I have had occasion to refer to EEG studies carried out during meditation as it is practised by priests of the Zen and Yogi sects—which I regard as a kind of hypnosis, or better, autohypnosis. In Japan some investigators, e.g. KASAMATSU and HIRAI (1966), have carried out some very good work on the EEG pattern during Zen contemplation. In Zen meditation a very special posture is adopted by the priests: in order to obtain a kind of sensory deprivation, they sit in a quiet room with their legs crossed (the "Lotus position", which is one of muscular relaxation) keeping the *eyes open* and fixed on some object. In this posture, they enter upon their meditation—or rather, a particular condition which is characterized by the absence of mental activity. At the beginning of this state, where the subject's eyes are open, there is no alpha activity; but, after fifty seconds or one minute, rhythmic waves in the alpha range appear. This alpha activity is very marked, diffused all over the scalp, and can be of long duration. In those priests who have had the most advanced training—one might say, the masters—in from one to three minutes the alpha waves decrease in frequency, while their amplitude increases, and eventually, in some subjects, they are replaced by pure theta activity.

If, in the course of Zen meditation, a click is presented during the period of alpha activity, there is a brief desynchronization. The regular repetition of such stimuli does not, however, induce habituation. In a normal subject habituation occurs after three to six repeated stimuli, but in this particular state, even after application of the stimulus as many as 50 times, there persists a blocking effect which excludes any habituation. Is it possible to compare EEG findings in drowsiness with those recorded during Zen meditation? Certainly there is at least one difference: normally in a state of drowsiness the EEG pattern of alpha activity does not last as long as that which may occur during meditation.

During drowsiness alpha activity soon disappears, but in Zen meditation it may persist for half-an-hour without any change. I do not know whether this is a pre-hypnagogic state which does not progress to sleep. It is, however, important here to refer further to another aspect of sleep. If one awakens young people and children from the light phase of sleep or the 1-REM stage, one obtains an arousal pattern, followed by a desynchronized EEG. But if, on the other hand, one awakens them from a very deep sleep (principally, stage 4), there is very often, though not always, prolonged alpha activity—sometimes for 2 to 4 min—despite the fact that the subjects' eyes are open. On the scopogram, there is a lot of blinking movement and a lot of scanning movement. The subjects' eyes are open, and yet there is no blocking of alpha activity. Even if one claps one's hands, there is still no blocking effect. I have called this phase "1a3", but it cannot be regarded as drowsiness because it appears only when the subject is awakened from deep sleep. Stage 1a3 can be differentiated from stage 1a1, because the stimulus of a click desynchronizes the alpha activity of stage 1a1, but not the alpha-like activity of stage 1a3. For this reason, stage 1a3 cannot be identified with the EEG pattern encountered during Zen meditation, since in the latter every click desynchronizes the alpha rhythm.

The long duration of alpha activity is, however, common both to Zen meditation and to phase 1a3. Two or three EEG studies have been undertaken on subjects during Yoga meditation, and I myself carried out one such investigation many years ago. In particular, together with Dr. DAS of Calcutta (DAS and GASTAUT, 1955), I studied members of a Yogi sect of Bengal in the state which they call "Samadi", which is a kind of ecstasy. I made recordings of this type of trance, which occurs with the *eyes closed.* First, there is alpha activity; and when they begin meditation and then achieve Samadi—(it is very difficult to explain how this state is attained)—there is a desynchronization, as though the attention was focussed on something. No other research workers obtained results similar to mine in studying these people. The investigators who were interested in making recordings of the Yogi did not study the Yogi of southern Bengal, but those of the mountains in Kashmir. Other authors, e.g. BAGCHI and WENGER (1959), have found, throughout this type of meditation, the same persistent pattern of alpha activity. However, ANAND *et al.* (1961) in India state that during this stage a clap of the hands has absolutely no effect—exactly the reverse of the findings of other investigators. According to them, alpha activity continues in precisely the same way as I have found it to do in phase 1a3, when subjects are awakened from deep sleep: there is no reaction.

Another point is that many of those who practise Zen or Yogi meditation, do so with a view to attaining a sort of ecstasy—a cosmic ecstasy. This same term is employed by people who use psychedelic drugs, and it is very interesting to note that with all the psychedelic drugs—and in particular LSD, which is the most effective—the EEG findings are exactly the reverse of those occurring in the aforementioned states, i.e. there is an acceleration of the alpha rhythm and a sort of desynchronization. It is not a true desynchronization but in actual fact an increase of the alpha rhythm from 10 to $11^1/_2$, or sometimes 12. It would be very interesting to know why some people (such as, for example, the Beatles) abandon the use of marihuana for Yogi practices. The two conditions thus induced, respectively, seem to be very different according to EEG findings, because there is no doubt whatsoever that in hypnotic states there is increased alpha activity, whereas the action of psychedelic drugs is exactly the reverse.

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A Critical Evaluation of the Pavlovian Theory of Hypnosis

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The work of Pavlov has been strongly influential in Eastern Europe, in research and theory concerning hypnosis, and has also influenced some research workers in Western countries. Because of the possibilities which it offers for providing explanations of the problems involved, this theoretical approach remains the focus of a continuing interest on the part of investigators.

Pavlovian teaching in general is from time to time evaluated, and indeed criticized, in books and articles published in English (Konorski, 1948; Rahmani, 1963; Astrup, 1965; Hilgard, 1966). The best known evaluations and criticisms of the Pavlovian theory of hypnosis are those of Hull (1933), Kubie and Margolin (1944), Ingham (1956), and Das (1958). (The articles by Kubie and Margolin, and by Das, were reprinted in Shor's and Orne's "Readings", 1965, pp. 217—233 and 255—266.) Instead of reviewing past evaluations, this paper attempts to offer some additional remarks on the extent of the contribution of Pavlovian theory to the explanation of hypnosis.

Exposition of the Pavlovian Theory of Hypnosis

Before reviewing some general postulates of the Pavlovian theory of hypnosis, it must be noted that in the Soviet literature certain highly specialized meanings are habitually attached to terms deriving from the Pavlovian school and have become deeply integrated in Soviet thought and expression.

Excitation and *inhibition*, as well as *positive* and *negative induction*, represent some of the basic hypothetical concepts used by physiologists of higher nervous activity and psychologists of the Pavlovian school.

Excitation is viewed as a process manifested in the initiation or intensification of some function of the organism.

Inhibition in the Pavlovian framework refers to the opposite process, involving decrease or cessation of the function.

In this context, it is essentially central nervous and, more specifically, cortical processes which are postulated.

Induction refers to the influence of one cortical centre on another cortical centre or centres. The influence may be either *excitatory*, which is associated with *positive induction*, or *inhibitory*, which is associated with the process of *negative induction*.

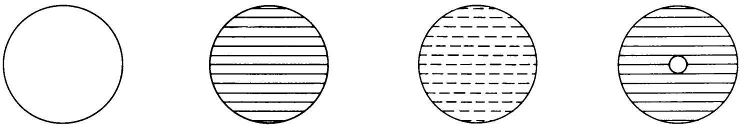
According to Pavlovian views, the "*watch point*" is a more or less confined centre of concentrated excitation, isolated from the remaining regions of the cortex by negative induction.

Excitation-Inhibition Propositions of Hypnosis

Proposition 1. — *Hypnosis is incomplete sleep, i. e. sleep accompanied by partial wakefulness.* Suggestion causes a concentration of excitation in part of the cortex, while other areas are inhibited by negative induction. This diminishes the effect of influences which would otherwise tend to counteract the suggestion. PAVLOV's theory implies that some degree of inhibition should be present following a period of suggestion, and that the amount of inhibition will to some extent determine the response to suggestion. PAVLOV stated that, just as we learn to go to sleep when we close our eyes, so too an individual may be taught to enter into an hypnotic state as a conditioned reflex to the appropriate stimuli, and also pointed out the influence of repetition. Suggestion is defined as concentrated excitation in a limited point or area of the brain, induced by a word, as a conditioned-reflex stimulus in the framework of the second signal system or, as PLATONOV (1959) explained suggestion, as a physiological factor.

Proposition 2. — *The dissociation of cerebral functions is held to be an important characteristic of the hypnotic state.* In other words, there is a subdivision of the cerebral hemispheres into sleeping and waking parts, and there also exists a "watch zone", a more or less confined centre of concentrated excitation isolated from the remaining regions of the cortex by negative induction. According to the Pavlovian school, this "watch zone" represents the physiological foundation of hypnotic rapport. For Pavlovians, hypnotic rapport is the special faculty of a hypnotized subject to perceive, selectively and exclusively, the words of the hypnotist without maintaining any contact with the rest of the external world. Pavlovians point out that this is not an exclusive property of the hypnotic state and that it is sometimes also observed in normal sleep.

In the accompanying diagram, in which shading represents inhibition and white areas represent excitation, an attempt is made to illustrate schematically: (a) sleep; (b) what is encountered as a popular misunderstanding of Pavlov, certainly inherent in Hull's writings (1933); and (c) the real Pavlovian position. Thus, it is necessary to distinguish between the concept of an incomplete irradiation of inhibition spreading over the entire cortex (a misconception especially prevalent in the West), and that of dissociated areas of excitation surrounded by irradiated areas of complete inhibition.



Wakefulness

(a) sleep

(b) partial inhibition view of hypnosis

(c) dissociated excitation-inhibition view of hypnosis

Developmental Propositions of Hypnosis

The Pavlovian school views hypnosis in the perspective of the *phylogenetic continuum.* It postulates that hypnotic behaviour in man is more complex because of the greater complexity of the human brain, as compared to the brain of lower animals.

Proposition 1.—*Animal hypnosis is a biological antecedent of human hypnosis.*

Animal hypnosis is considered to be a reaction which in lower evolutionary forms is a normal, more or less adaptive function of the animal to its environment, while in higher evolutionary forms it loses this significance and is rather an atavistic reaction.

Proposition 2. — *The ontogenetic development of the susceptibility to animal hypnosis seems in general to be identical with its phylogenetic development.*

The investigation of susceptibility to animal hypnosis in the phylogenetic line (lizard, cock, rabbit, cat) shows that animal hypnosis lasts longest in reptiles and birds. In the rat, susceptibility to animal hypnosis decreases in the course of individual development (Hoskovec and Svorad, 1965).

Evaluation and Criticism of the Pavlovian Theory of Hypnosis

It is clear that Pavlovian theory is in conflict with most theoretical conclusions derived from experimental work carried out in the West.

Pavlovian theory (Pavlov, 1923a, 1923b, 1927, 1928, 1941; Platonov, 1959), originally based on experiments with animals (dogs), was extended to include an explanation of human hypnosis. Some authors dogmatically adhere to the conceptualizations of a brilliant thinker and experimental scientist who, however, derived his theory of hypnosis from the knowledge of Russian "behavioural" physiology at the beginning of the century. It is only fair to Pavlov to add that he emphasized the necessity of carefully evaluating the significance of experiments with animals when applied to human beings. Rokhlin (1963), Korotkin and Suslova (1965), Slobodyanik (1966), and others still conceive of his teaching as the sole theoretical framework capable of explaining human hypnotic behaviour, although, perhaps also because of Pavlov's special emphasis on animal research, it is very largely the physiological aspects which are dealt with by these authors. They did, however, subsequently also show a certain concern with somewhat more psychological concepts.

The conditioned reflex can throw some light on hypnosis. Having introduced the conditioned reflex to hypnosis, Pavlov attempted to explain the mechanism of induction by rhythmic stimulation. In man, at least, from earliest infancy rhythmic movements are used by the mother to establish a primary conditioned reflex, inducing sleep, in which fatigue is the unconditioned stimulus, sleep the unconditioned response, and stroking and patting, with physical contact as well as rocking and even a lullaby, are the rhythmic conditioned stimuli. Phylogenetically, however, constriction apparently also constitutes an unconditioned stimulus, producing the unconditioned response of hypnosis. It is therefore possible that, through the constriction of swaddling in infancy, a second conditioned reflex is set up in which rhythmic stimuli, especially in the absence of fatigue, take over from constriction to induce hypnosis as the conditioned response. The technique of hypnotic induction may possibly be derived from this simple system. The question remains, however, whether swaddling in infancy does in fact produce a degree of hypnosis (Black, 1967).

The Pavlovian theory of hypnosis as applied to humans limits the work of the orthodox pupils of this school. Generally speaking, the reflexological aspect (conditioning) of this theory is fruitful, but at the same time the theory itself hinders the exploration of such non-physiological intervening variables as, for instance, personality variables, preconceptions of the subjects as to how they would behave in the role of the hypnotized person, etc.

The reader insisting on a rigorous differentiation between description (observations) and interpretation is sometimes dissatisfied by the short-circuiting between factual data and explanatory constructs. Let us say, for example, that a man is learning during hypnosis: to the orthodox Pavlovian, the perception and memorizing of speech under hypnosis are carried out through the functioning of the cells of the "watch point" or "watch zone" — sometimes also termed "guardian point" or "guardian zone" —

with simultaneous inhibition of the other areas of the brain (cf. e.g., SVYADOSHCH, 1962; and KULIKOV, 1964).

The use of the term "hypnotic sleep" by Pavlovians, instead of that of "suggested sleep", betrays a close affinity with the Pavlovian position which biases the issues under discussion and implicitly presupposes a close affinity between the hypnotic state and physiological sleep.

Experimental work outside the Soviet Union leads us to the conclusion that hypnosis is not necessarily connected with a change in the level of vigilance in the direction of sleep, as Russian authors claimed to have found in the EEG (cf. e.g., NEVSKII, 1963).

Electroencephalographic findings are especially important in this connection. Some authors (KRÁKORA, 1953) showed that there were no observable differences from the waking pattern in the EEG while the hypnotist was speaking with the subject, or while some manipulation was taking place under hypnosis. After a certain time interval following the last suggestion, however, the typical sleep pattern set in. If the EEG showed a deep sleep plattern, then the subject was not observed to follow a given suggestion in that period. However, some EEG recordings (ZIKMUND, 1964) show a superficial sleep activity in hypnosis.

Research on the EEG and hypnosis requires further elaboration and evaluation, with special reference to the more differentiated stage I (non-REM sleep).

Also, in the future more advanced research on single unit responses may be expected to provide the factual neuro-physiological grounds for the basic hypothetical constructs in the excitation-inhibition propositions referred to above.

While some authors in the Soviet Union still dogmatically adhere to a definition of hypnosis according to strict Pavlovian teaching, current empirical findings tend to suggest three possibilities, along the following or similar lines: (1) From the waking state to suggested sleep (called by some Russian authors "hypnotic sleep"), when suggestions of sleep are predominantly used and/or sleep is expected by the subject. (2) From the waking state to so-called passive hypnosis (according to WHITE, 1941a, 1941b). In both conditions, the excitation-inhibition propositions might be applicable. (3) From the waking state to so-called alert and active hypnosis (according to WHITE), with an altered level of consciousness defined by the presence of a particularly concentrated state of selective attention. This condition can hardly be explained by the Pavlovian point of view.

The aspects discussed in the present paper represent a limited part of a multidimensional approach to hypnosis which cannot be treated in a purely Pavlovian perspective.

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Some Remarks on "Passive" Concentration and on Autogenic Shift

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For some years we have been interested in the psycho-physiological and neuro-physiological aspects of autogenic training. In our previous published papers our attention was centred on two major problems: the pattern of the EEG and vegetative patterns during the standard exercises of autogenic training, and the EEG and vegetative reactivity during the autogenic state (JUS and JUS, 1960, 1963, 1965a, 1965b, 1965c; LUTHE *et al.*, 1963). In our present studies, we are mainly concerned with research on "passive" concentration and on the nature of the autogenic shift itself.

It may be relevant, in the first place, to summarize briefly our earlier findings: In the study of the EEG structure and vegetative patterns during the standard exercises of autogenic training, we confirmed the observations of ROHMER and ISRAEL (1957) on two principal types of evolution of the EEG recording. In subjects with an initial abundant alpha rhythm with regular waxing and waning, this was succeeded by a "fractioned" alpha activity and random theta activity, first in the anterior leads and later becoming generalized, but always superimposed on a background of alpha activity. In subjects with dominant beta rhythm and poor alpha activity, the evolution first went through the stage of increasing abundance of alpha activity, followed by the same stages as mentioned above, to the stage of generalized theta activity. The stage of anterior theta activity was usually associated with the sensation of heaviness, and that of generalized theta activity with the sensation of warmth. The shift toward these sensations, which represent the stages of the first two exercises of autogenic training, was attained more rapidly in subjects with an abundant alpha activity, than in those with initial beta activity.

At the same time, the abdominal type of respiration (in men) changed to the thoracic type and the rate of respiration decreased. During the first two standard exercises, the heart rate also decreased and the GSR curve remained isoelectric.

Reactivity studies had to be very delicately performed in order not to impede the evolution of the autogenic state. Thus, they were chiefly conducted at the stage when slow EEG activity appeared. The observed response on this background was similar to the arousal reaction in the first stages of sleep: abundant alpha activity superimposed on the background of slow rhythm. We have noted that the reactions may be present in one, in several, or in all polygraphic curves, and they may or may not be accompanied by sensations. Thus, for instance, a click may provoke an arousal in the EEG and/or one or more vegetative reactions, and the perception of the click may be either present or absent. There are several possible combinations of sensations giving rise to polygraphic reactions, and the comparison of sensations with polygraphic reactivity is of special interest. The study of these correlations is, in our opinion, the best source of information on the state of consciousness of the trainee.

The fluctuations in correlations between perception and polygraphic reactivity are very characteristic of this stage of autogenic training. Here, the GSR seems to be a good indicator. When the GSR is present, perception is almost always present; when the GSR is absent, perception may be present or absent.

In further research on the same trainees, and on 8 new short-term trainees, we were especially interested in the problem of "passive" concentration and the mechanism of the autogenic shift.

In J. H. SCHULTZ's (1956) original concept, the psycho-physiological shift (*Umschaltung*) from the normal to the autogenic state is a specific process involving changes in mental and bodily functions which enable a person to "step behind" or "dive under" the usual pattern of those functions associated with conscious control in the waking state.

However, this shift can assume different forms. In collaboration with LUTHE and GEISSMANN, one of the authors has already put forward the view that different forms of autogenic shift are possible (LUTHE *et al.*, 1963). This hypothesis was based on the EEG differences in short- and long-term trainees (cf. JUS and JUS, 1965a). In short-term trainees, or in the learning phase, the autogenic shift may take place at the stage of "pre-drowsiness", whereas in well-trained subjects the shift would occur almost immediately after the waking state. We have also put forward the hypothesis that a direct shift from a state of drowsiness to the autogenic state is not possible, and that the psycho-physiological change from the autogenic state to sleep passes through the stage of "pre-drowsiness". Our present studies have led us to the conclusion that there are yet other different forms of autogenic shift. To understand this, it is necessary to analyse in greater detail the concept of "passive" concentration, because the latter is believed to be the process by which the autogenic shift is accomplished.

In this connection, SCHULTZ and LUTHE (1959) may here be cited: " . . . 'passive' concentration . . . may best be explained in contrast to what usually is called concentration or, more precisely, 'active' concentration . . .

The decisive difference between this usual type of 'active' concentration and the 'passive' concentration used during autogenic exercises lies in the person's attitude toward the functional result to be achieved. 'Passive' concentration implies a casual attitude and functional passivity toward the intended outcome of his concentrative activity, while 'active' concentration is characterized by the person's concern, attention and goal-directed active efforts during the performance of a task, and in respect to the final functional result".

This is absolutely true, and evidence in support of this statement is provided by polygraphic and introspective data, but only in so far as the first two standard exercises (heaviness and warmth) are concerned. The problem becomes much more complicated in the case of exercises associated with voluntary control of vegetative functions. In the case of the sensation of heaviness, the subject has to induce muscular relaxation by getting rid of muscle tension. He simply has "to relax". It is not difficult to connect the sensation of muscle relaxation with a feeling of heaviness and a reduction of mental activity. These phenomena are already known to occur on occasion in everyday life, in the process of falling asleep or resting when one is tired. After physical tension associated with mental effort directed towards an efficient performance, human beings have frequently experienced rest. The state of "rest" is nothing else than muscular relaxation, combined with the sensation of heaviness in the limbs and mental relaxation. A "passive" concentration on this "non-action" is understandable. It is a little more difficult to apply the expression "passive" concentration to the second standard exercise consisting in attaining the sensation of warmth. However, as SCHULTZ and LUTHE (1959) themselves observe, there is a close relation between the mechanisms for regulating the tone of the striped muscular system and other mechanisms for vasoconstriction and vasodilatation. A reduction of the tone of striped muscle automatically induces changes in the peripheral circulation, and the consequent peripheral vasorelaxation is accompanied by the subjective feeling of warmth. As in the case of the sensation of heaviness resulting from muscular relaxation, the subject has here similarly to concentrate on physiological phenomena which are automatically induced after muscular relaxation. Further, it is easy to concentrate "passively" on the non-active, passive state of relaxation.

However, it becomes extremely difficult, if not impossible, to operate with the concept of "passive" concentration, in the case of voluntary regulation of vegetative functions. Already, the statement that the average trainee must be helped to "discover" his heart implies the necessity of "active" concentration. There is no doubt that in slowing or increasing the heart rate an active effort is required. This is no longer the "dolce far niente" of the first two standard exercises, where the position of rest automatically induces sensations of heaviness and warmth and consequently mental relaxation. On the contrary, the third exercise is an action in a definite direction, requiring an important concentration of attention on vegetative functions. Evidence in support of this statement is provided by polygraphic and introspective data. In the experiment in which the trainee himself indicates his readiness for the subsequent stages of autogenic training, the exercise of accelerating the heart rate is, even in long-term trainees, always anticipated by an EEG arousal and an alteration in respiration. Such changes do not occur in long-term trainees prior to their entering upon the first two stages of autogenic training (heaviness and warmth). Sometimes also a drop in the GSR curve anticipates the third exercise. The introspective data of the personal account provided by the trainees accords with the physiological phenomena observed. In all cases, the trainees reported a quite different mental attitude during the performance of the first two exercises (heaviness and warmth) from that occurring during the exercises concerned with visceral regulation, starting with the exercise of regulating the heart rate. Whereas, during the first two exercises, the trainee "dived" —in SCHULTZ's expression—into a hypnotic-like state which we call the autogenic state, before the exercise of voluntary regulation of the heart rate he "emerged" from this state, merely to "trigger" the voluntary action. Of course, after prolonged training there was some automatization and perfection in achieving voluntary visceral regulation, but the "shift" to the visceral exercises comprised a very different experience, as compared with the "shift" to the first two exercises.

It seems to us that this is in accordance with the therapeutic concept of autogenic training. It is necessary to distinguish two kinds of therapeutic application of autogenic training. One particular kind of exercise is recommended where the therapist is of the opinion that in a given case "pure" relaxation is necessary; and a different kind of exercise is recommended when a regulation of visceral functions (consequent upon former training in relaxation) is indicated. In the first case, the therapeutic effect is achieved already by the performance of the first two standard exercises; in the second case, further steps toward visceral exercises are necessary. This would imply in the first case treatment by "rest", by "relaxation", and by "passive" concentration on a "relaxed", "heavy" and "warm" body, with subsequent mental relaxation. And in the second case, it would involve treatment by "training", "exercises", and "active" concentration on "regulating" visceral functions. We feel that the expression "relaxation" should be confined to the first two exercises of autogenic training. It is rather to the later standard exercises that the term "autogenic training" is essentially applicable. These are truly training exercises, and they do in fact constitute a real, as well as an autogenic, training, but an active one.

In the light of these observations, it would seem justifiable to differentiate at least two kinds of autogenic shift: the shift induced by the method of "passive" concentration on the relaxed body, and the shift indnced by the method of "active" concentration on regulating voluntarily the vegetative functions. The first type of shift would involve "diving" into the state of relaxation attained by the first two exercises of heaviness and warmth. The second type of shift would imply "emerging" from the state of relaxation with enough strength to "trigger" the voluntary regulation of the vegetative functions. The state of relaxation before visceral exercises constitutes an ideal starting point for further exercises. The concentration on verbal formulae of relaxation, heaviness and warmth, is an excellent preparation for subsequent procedures by which the subject is "mobilized" for voluntary visceral exercises.

Summary

The authors describe the results of their research on concentration in autogenic training. Through polygraphic recordings, it was possible to establish a substantial difference between the "passive" concentration accompanying the shift to the first two stages of autogenic training, concerned with relaxation (heaviness and warmth), and the "active" concentration associated with the shift to the subsequent stages, concerned with the subject's voluntary regulation of his vegetative functions. Long-term trainees show no changes in polygraphic curves immediately before, and when entering upon, the first two stages; but, in the same trainees, very distinct changes are recorded in the EEG (arousal) and in other polygraphic curves immediately before entering upon the stages of regulation of vegetative functions. Similarly, a different mental attitude is present during concentration in the first two stages, and in the subsequent stages, respectively. It is concluded on the basis of polygraphic and introspective data that, in each of these instances, the autogenic shift has a different character: it is accompanied in the first two stages by passive concentration, and in the stages of regulation of vegetative functions by active concentration.

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Peripheral Changes in Blood Circulation during Autogenic Training and Hypnosis (Results of Experimental Research)

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In addition to the immobilization of the limbs and the associated sensation of heaviness, most patients in a hypnotic trance also experience a sensation of warmth. This subjective feeling is probably accompanied by increased skin temperature. Attempts have been made in the past to record skin temperature objectively. Most methods of measurement, however, are so defective that they have only rarely proved at all reliable. HENSEL (1955, 1964) and his colleagues have described seven variables whose range is so great that the statistical evaluation of such measurements becomes impossible, even though the data may be valid in individual cases. Furthermore, graphic recording is also impracticable, as any prolonged application of the probe to the skin distorts the recorded values.

This being the case, the author has attempted to devise new methods of measurement. Two methods were available: ASCHOFF's system of calorimetry, which, however, similarly does not allow any extended recording of measurements; and HENSEL's method of measuring the degree of "heat conveyance" (*Wärmetransport*). As it was decided to use HENSEL's method, it is perhaps relevant here to give a brief description of its mode of action.

Because of the essentially linear relation of the degree of heat conveyance to the skin's circulatory changes, it is preferable to employ a thermo-electric method of measurement. HENSEL's method makes it possible to determine the direction, duration and extent of any circulatory changes without in any way affecting the patient. The apparatus consists of an adjustable electrical heating system which is coupled with a thermo-electric system of measurement. This so-called "convection sensor" measures the temperature difference between a small, heated metal plate and a closely adjacent unheated plate. The heated plate transfers a certain amount of heat per time unit to the medium, in proportion to the increased temperature and the conductivity of the medium in contact with the plate. When the two plates are placed in direct contact, the temperature difference is minimal. Thus, by this method it is possible to determine the degree of thermal conductivity in the skin. Tissues in which blood is circulating transmit heat from the heated plate, not only by conduction but also by convection. We are not, therefore, measuring thermal conductivity as such in the physical sense, but rather a quantity which may be called the degree of heat conveyance. We can determine the degree of heat conveyance of skin in which there is no circulation, as well as that of skin in a state of inactivity. If we subtract the former value from the latter, we obtain a value ($\Delta\lambda$) that provides a relative indication of the degree of blood circulation. The degree of heat conveyance has the following dimensions: $\text{cal} \times \text{cm}^{-1} \times \text{sec}^{-1} \times {}^{\circ}\text{C}^{-1}$.

For some time, the literature on this subject was involved in a controversy that centred around attempts to arrive at an absolute measurement of circulation from the degree of heat conveyance. This controversy does not, however, affect the author's work, as he is not concerned with absolute figures, but rather with comparisons. The graphs show variations corresponding to a spontaneous rhythm which seems to be determined by the capillaries. In addition, there are somewhat larger variations that indicate an influence exerted by the patient's respiration.

Individual series of experiments were undertaken with continous recording of the degree of heat conveyance.

I. Together with Vogel (1967), the author investigated the changes in peripheral circulation during the learning of autogenic training, with the following findings:

1. If the patients lie quietly for a sufficiently long period of time in a room kept at constant temperature, there is no change in the peripheral circulation (measured on the thumbs). However, once these patients begin the relaxing exercises of autogenic training and begin to experience a sensation of heaviness, a close relation develops between the time period and an increase in the degree of heat conveyance, which in turn would seem to indicate vascular dilatation. Here, characteristic curves were also obtained, which were subsequently found by other workers.

If the patient begins with concentrated self-relaxation, the degree of heat conveyance decreases for a short period (the initial decrease), which corresponds to a short-term vasomotor constriction. Then, however, the "main reaction" in the increase of the degree of heat conveyance (a) begins and (b) continues throughout the entire period of autogenic training. During the final stage (which here involved only leg movements and deep breathing, so as not to disturb the metal plates on the thumbs) we measured (c), a simultaneous sudden decrease in the degree of heat conveyance which was followed by (d), a slowly-developing after-reaction, until the normal degree for a quiescent state was reached some 15 min later (Fig. 1).

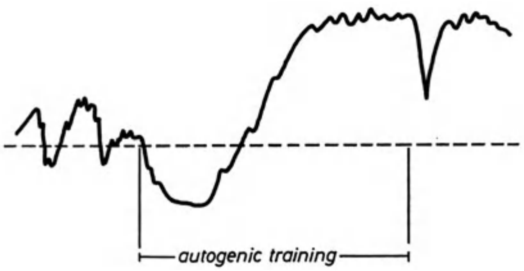

Fig. 1. Typical development of heat conveyance during autogenic training

2. The vasodilatation of the main reaction starts at the beginning of the first week of training, and increases progressively up to the fifth week. With the introduction of the special "warmth" exercise, the curve shows only a slight increase (see Fig. 2).

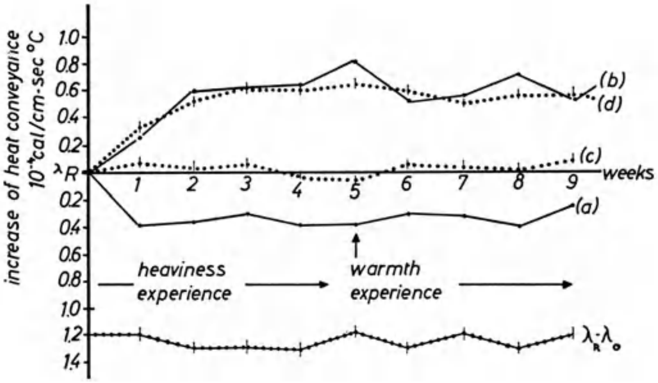

Fig. 2. Mean deviation of heat conveyance (λ_E) during 9 weeks of training: 4 typical curves (a – d)

Thus, immobilization of the muscles already leads to vasodilatation in the subcutaneous blood-vessels. This was not expected, for the muscular blood-vessels are not connected with those of the subcutaneous tissue: in fact, they usually react in an opposite way.

We may therefore conclude that vasodilatation and its accompanying sensation of warmth during autogenic training have a physiological correlation. They are not a product of the imagination and, consequently, are not simply a psychological phenomenon. One could even say that this peripheral vasodilatation begins much earlier than was previously supposed and thereby represents an aspect of the "psychobiological shift to a tropotrophic state" (LUTHE).

3. These systematic investigations showed several interesting features: The changes in the degree of heat conveyance were always symmetrical

and never showed any lateral differentiation. These findings were constant, and were independent of any unilateral experience of a sensation of heaviness or warmth. And even when the arm used in the experiment alone felt warm, the vasodilatory reaction was nevertheless symmetrical (Fig. 3). Thus it becomes clear that the vasodilatation observed in autogenic training follows neurophysiological laws and is symmetrical because of the reflex nature of the process. The experience of unilateral warmth, on the other hand, is not a physical phenomenon, but a psychological effect traceable to the range of the focus of attention.

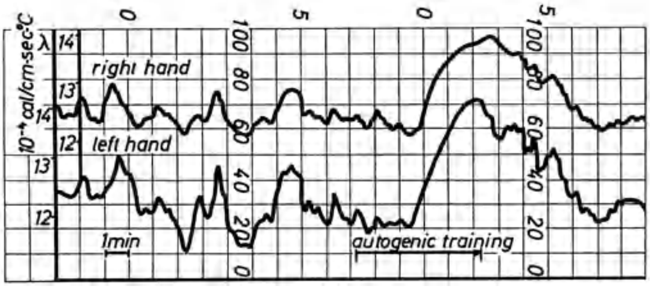

Fig. 3. Symmetric curves of heat conveyance in both hands before and during autogenic training (although warmth imagined in one hand only)

II. The above results were supplemented (in cooperation with Uber, 1966) by a comparison of the degree of heat conveyance during autogenic training and graduated active hypnosis.

Patients who were well advanced in autogenic training showed, on average, a more prompt and more clearly defined increase in the degree of heat conveyance, as compared with those undergoing graduated active hypnosis. The latter group showed a slower and less prompt increase, even when subjected to a very definite suggestion of the sensation of warmth (Fig. 4.)

These observations complement those made by Vogel (1967). Peripheral vasodilatation, consequent upon a concentrative introspection, is not only an autosuggestive process, but is actually a physically determined reactive process. These processes are not nearly as pronounced when they are initiated through verbal suggestion, as, for example, in graduated active hypnosis, even when the subjective experience is more intense. Thus, suggestion is much more than just a psychological process, whereas in autogenic training vasomotor activity is based on a physical reaction.

These results indicate the extent to which autogenic training depends not only on suggestive-autosuggestive factors, but also on physical factors. Because of the importance of this question, it is intended to repeat these experiments, but this time within the framework of pure hetero-hypnosis.

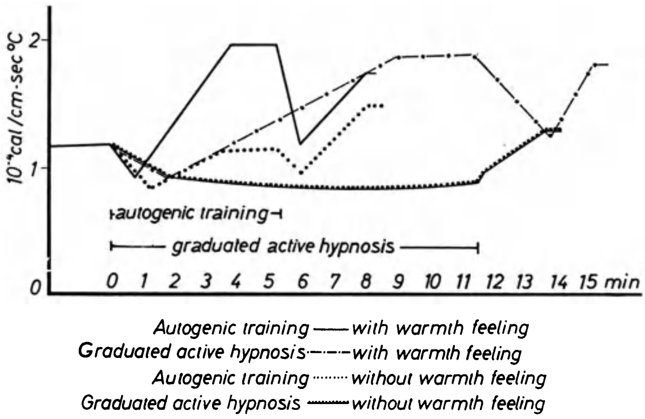

Fig. 4. Development of heat conveyance on top and at base of thumb

III. A further series of experiments was undertaken (in cooperation with SCHWARZ, 1967), to test vasoregulation in autogenic training and in hypnosis at low room temperatures.

The peripheral cutaneous circulation is influenced primarily by environmental temperature. The lower the temperature, the greater the vasoconstriction. Below the so-called "zone of body cooling" (10°—15C° room temperature) no appreciable decrease in circulation can be detected. We can claim to have reached this state in our experiments, as they were all carried out at room temperatures of 0°—10°C. Under such conditions, as opposed to autogenic training at18 °—20°C, vasoconstriction is no longer normal, but has reached a maximum. Relaxation and vasodilatation become more difficult to achieve, because of the painful effect induced by the cold.

Experiments were carried out with 12 subjects (principally students), all of whom had been well or thoroughly acquainted with autogenic training for periods of 2 to 5 months. In all, a total of 168 continuous graphic recordings of the degree of heat conveyance were made. In these experiments, the subject usually remained for an average of two hours in room temperatures of 0°—10°C. After such a period, the degree of heat conveyance corresponded to the minimum circulation in a relaxed state.

The recorded curves approximate to those already shown in Fig. 1. Individual differences are, however, apparent in Fig. 5.

1. The increase in the degree of heat conveyance was often more pronounced when compared with the initial degree which, because of the cold, was very low. Often the initial degree was tripled. During the experiment some subjects sweated on the palms of their hands.

2. Even under the influence of cold, the spontaneous rhythm of the small vessels (which had been lost as a result of vasoconstriction) reappear-

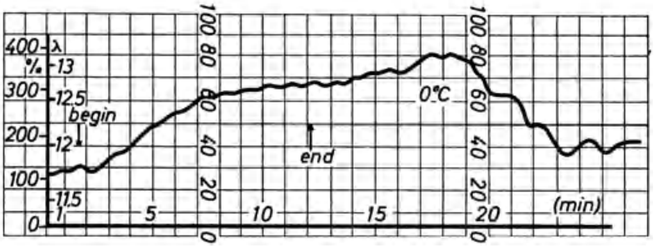

Fig. 5. Increase of heat conveyance during autogenic training at low room temperature

ed, so that blood-pressure waves of the third order could be seen on the graphs. Once the tissue has been rewarmed through vasodilatation, the smaller vessels are able to respond proportionately by changes in calibre. This corresponds to blood-pressure waves of the third order.

Fig. 6 shows the extent to which all physiological processes are slowed down by lower temperatures. The maximum increase in the degree of heat conveyance is reached twelve months after the start of an autogenic exercise. At times it is reached only after the exercise has ended. Normally, the increase reaches its maximum a short time after the exercise has begun.

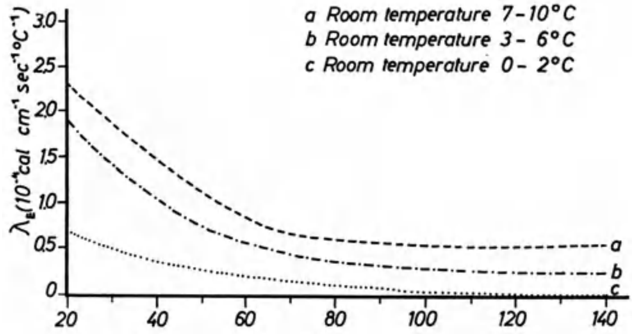

Fig. 6. The influence of the duration of low temperature on the increase of heat conveyance (λ_E) during autogenic training

In the evaluation of the final results it was also possible to identify several characteristic effects:

An increase in the degree of heat conveyance clearly depends on the duration of exposure to cold. This is shown in Fig. 6. The longer the subject remained in a cold room, the less was it possible, in spite of autogenic training, for vasodilatory impulses to exert an influence. This reaction is, in fact, also proportionate to the temperature. The colder the room, the more difficult is the achievement of vasodilatation. Even the return to the initial degree of heat conveyance clearly depends upon the external temperature. This is apparent in the following table:

Table 1. *Return to initial degree of heat conveyance after termination of autogenic exercise, as influenced by room temperature*

Room temperature	Time required after termination of exercise to return to initial degree	Number of subjects	Standard deviation
7—10°C	11.3 min	N = 56	± 3.6 min
3— 6°C	8.3 min	N = 28	± 2.7 min
0— 2°C	1.8 min	N = 32	± 2.5 min

Not all subjects, in spite of their good command of autogenic training, were able to perform the "warmth exercise" equally well. In 168 experiments, made with 12 subjects, vascular reactions were absent

at room temperatures of 0— 2°C, in 45.5%
3— 6°C, in 37.5%
7—10°C, in 27.0%

That an auto-hypnotic state did in fact occur, was shown by muscular hypotonus, as well as by a positive jaw phenomenon, and by the alteration in respiration.

These results indicate that the reaction to the experience of cold was the decisive factor. If the subject succeeded in developing indifference to a painfully experienced low temperature, he was then able to introduce the "warmth exercise" together with its concomitant vasodilatation, which is a learned, as well as a reflex, process. If the subject did not succeed in becoming indifferent to cold, it proved impossible to cut off the vasoconstrictor impulses affecting thermoregulation.

It is therefore evident that the varying responsiveness to different room temperatures cannot be explained physiologically, since vasoconstriction is maximal at 7—10°C. Hence the point of departure for vasodilatation must remain the same. Consequently, we must be dealing primarily with a psychological phenomenon: painfully experienced room temperatures of 0—2°C can only be overcome be some 50% of the subjects. Higher temperatures, however, make it possible for a greater number of subjects to counteract the environmental temperature and thus for more subjects to achieve vasodilatation.

Further experiments are required to determine whether a subject who is well versed in autogenic training is better able to realize vasodilatation under hetero-hypnosis and suggested insensitivity to cold. This result is, however, to be expected.

IV. In addition to the work described above, the author would like to mention the study (in cooperation with HOHN, 1966) of an experimental group that was subjected to pharmacologically induced vasodilatation in combination with autogenic training. Vasodilatory drugs cause an appreciable increase in the degree of heat conveyance. At an external temperature of 21°C, this effect is caused by the opening of the arterio-venous anastomoses; and, at 22—30°C, by the dilatation of the capillaries. These drugs cause greater vasodilatation in persons who practise autogenic training, principally because dilatation of the vessels has already been achieved by repeated exercises. The sensation of warmth during an auto-hypnotic state is enhanced by certain drugs. Conversely, pharmacologically induced vasodilatation is increased when applied during an auto-hypnotic trance.

This was not the case when pharmacologically induced vasodilatation was combined with hetero-hypnosis. Consequently, these findings once again confirm the difference between a suggested feeling of warmth, and a vasodilatory change brought about by systematic auto-hypnotic exercises.

Summary

The determination of the degree of heat conveyance, according to HENSEL'S method, enabled us to measure circulatory changes during autogenic training and hypnosis, respectively, under various conditions.

1. Graphic records show a typical course of circulatory change, comprising an initial decrease, a main reaction, and a gradual decrease at the end of the autogenic exercise. Vasodilatation begins, in autogenic training, once the sensation of heaviness has been experienced. The more familiar the subject becomes with the exercise, the greater is the increase in vasodilatation. Vasodilatation is always symmetrical, even when a subjective sensation of warmth is felt on one side only.

2. Suggestions of warmth given during graduated active hypnosis do not cause as rapid or as great a vasodilatation as that obtained under autohypnosis, even though the sensation of warmth may be more intense in the former case.

3. During experiments at low external temperatures, the increase in peripheral circulation was greater, although it developed more slowly. The lower the room temperature, the less able was the subject to achieve vasodilatation. In this case, vasodilatation depends on whether the subject can develop indifference to cold. Under simultaneous hetero-hypnosis, no additional changes were observed.

4. Pharmacologically induced vasodilatation, and vasodilatation achieved through autogenic training, are complementary, so that the two methods are mutually effective in improving the reaction.

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The Role of Emotional Stress in the Hypnotization of Animals and Man

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The researchers who, three centuries ago, applied the term "hypnosis" to catalepsy in animals most probably did so because of the similar appearance of these two conditions. From the results of subsequent investigations, it is justifiable to regard the term "animal hypnosis" as highly appropriate. Motor inhibition in animals and the state of hypnotic trance in man appear to be genetically interrelated, have a similar adaptive significance and, in all probability, possess certain common neurophysiological mechanisms.

We would particularly like to stress that both hypnosis induced by forcible immobilization in animals and the state of enhanced suggestibility in man, occur in situations where their respective attempts to reach a goal are ineffective, whether it be a question of the recovery of a posture by the animal, or of the solution of those complex vital problems with which man is confronted. In a very general way, it may be said that in both cases one finds an acute lack of pragmatic information, necessary and sufficient to satisfy the present need of the subject. But it is precisely such situations that are extremely typical of emotional stress, with all its inherent vegetative and electroencephalographic symptoms.

Proceeding from experiments which it is not possible to describe in detail within the limits of an article, we have formulated a rule which postulates the dependence of the degree of emotional stress on the magnitude of the need, as well as on the difference between the information prognostically necessary and that actually available to the subject (SIMONOV, 1965, 1966):

$$E = -N\ (I_n - I_a)$$

where E is the emotion, $-N$ is the need, I_n is the information prognostically necessary, i.e. required for undertaking actions to satisfy the given need, and I_a is the information available, i.e. actually at the subject's disposal.

The term "information", as it is used here, involves its pragmatic importance, which may be defined as the change in the probability of reaching the goal in consequence of the reception of the given communication (KHARKEVICH, 1960).

In those cases where the degree of emotional stress is measured by the magnitude of vegetative shifts (such as, for example, accelerated heart rate), the need by the intensity of painful electrical stimuli or by the duration of hunger, and the lack of information by the magnitude of error in motor reactions, the validity of our formula has been experimentally corroborated.

Measuring the degree of emotional stress by the value of objectively recorded vegetative shifts makes it possible to assess the magnitude of the need, the lack of pragmatic information being known or constant. Incidentally, we are also able to estimate the magnitude of the need by the degree of volitional effort or, more specifically, those of its characteristics which can be recorded. It is not possible here to consider in any detail the psychophysiological mechanisms of will. We would merely note that will is the activity of a nervous mechanism by means of which logical analysis of a given situation and long-range prognosis of further events intervene in the competition between needs. This is why the degree of volitional effort is proportional to the difference between the magnitude (force) of competing needs.

From the physiological point of view, emotions are the activity of a special nervous mechanism which is involved in the process of adaptive behaviour when the information essential to the achievement of vital goals is lacking. In a certain sense it may be said that emotions make up for the insufficiency of rational experience. This compensatory role of emotions is manifested in diverse forms. In a state of emotional excitation, living beings pass to dominant modes of reaction, in consequence of which indifferent stimuli or signals, which formerly had a different significance, prophylactically begin to evoke orienting, defensive, sexual and other reactions. It should especially be emphasizid that it is here a question, not of supplementing the insufficient information (this occurs in the process of search and re-examination of the memory cells), but of urgently replacing the information that is lacking. When in a state of fear (defensive dominant) an animal responds, for self-preservation, to a large number of stimuli, including those which it encounters for the very first time, its behaviour does not proceed from the knowledge of actual signs that it is threatened and of ways of warding off the danger. Such knowledge as the animal possesses in this respect originates in a secondary way, from learning and selection, in consequence of a process which, as it were, goes counter to the emotion, and at a certain stage eliminates it. Fear (the dominant) is intended to replace and compensate the insufficiency of information about effective means of protection.

Another instance of urgent replacement of insufficient information is provided by transition to imitative behaviour of the mass panic type, which again is closely linked with the nervous mechanism of emotions. In a situation where a living being is incapable of independently assessing the predicament that has arisen and reaching a decision, or lacks the requisite time for this purpose, it is biologically expedient to rely entirely on the experience of other individuals and to proceed to a blind reproduction of their actions. Imitative behaviour undoubtedly has its limitations, and in a number of cases proves to be disastrous for the individual, but in the course of a lengthy evolution the gain appears statistically to have prevailed over the loss, thus leading to a consolidation of imitational reflexes in the arsenal of adaptive mechanisms.

Originating in a situation of pragmatic uncertainty, emotional behaviour is always of an overcompensatory nature and involves an excessive loss of energy. Hence the increased activity of the cardiovascular and respiratory systems, the endocrine glands, the higher centres of the brain, etc., which constitutes one of the most vivid and significant manifestations of emotional stress in animals and man.

Following these brief remarks on the genesis and biological significance of emotional stress, we may now proceed to an analysis of the hypnotic phenomenon, beginning with the hypnotization of animals by forcible immobilization. It is quite evident that the procedure of immobilization presents precisely the type of situation where the entire complex of emotional stress symptoms may be expected to arise. And indeed, this occurs not only at the moment when the animal shows motor resistance but is at the same time superimposed upon an already-developed cataleptic state. If the hypnosis is of short duration and the experimenter takes no special measures to deepen it, the immobility of the rabbit lying on its back is combined with clear signs of excitation: accelerated heart rate and respiratory rate, and the initiation of specific electroencephalographic activity (KLEMM, 1966; and our own experiments). It is only when the hypnotic state is protracted that a gradual normalization of vegetative shifts and bio-electric activity occurs.

Thus, there is every reason to regard animal hypnosis as a special case of an emotional reaction to unsuccessful attempts to maintain the normal position of the body in space. Selective inhibition of the motor area is a specific feature of this reaction. The biological expediency of a reaction of this kind is readily apparent, and was already clearly defined by PAVLOV (1949). When an animal encounters a powerful enemy, motor inhibition serves not so much to help the prey to remain unnoticed (for a predatory animal, guided by the sense of smell, this is quite unimportant), as to preclude its provoking aggressive activity on the part of the predator. It is well known that the degree of aggressiveness of most beasts of prey is not dependent in a linear way on the degree of their food motivation. A passively prostrated animal has a chance of avoiding an attack, whereas motor restlessness and, still more, signs of resistance are in themselves capable of provoking aggression even on the part of a satiated enemy.

Thus, animal hypnosis is a biologically expedient adaptive reaction, developed in the process of evolution, which is aimed at warding off aggressive actions on the part of a strong enemy. What then is the mechanism which ensures transition from active resistance to motor inertia? What is the nature of the inhibition which arises in this particular case?

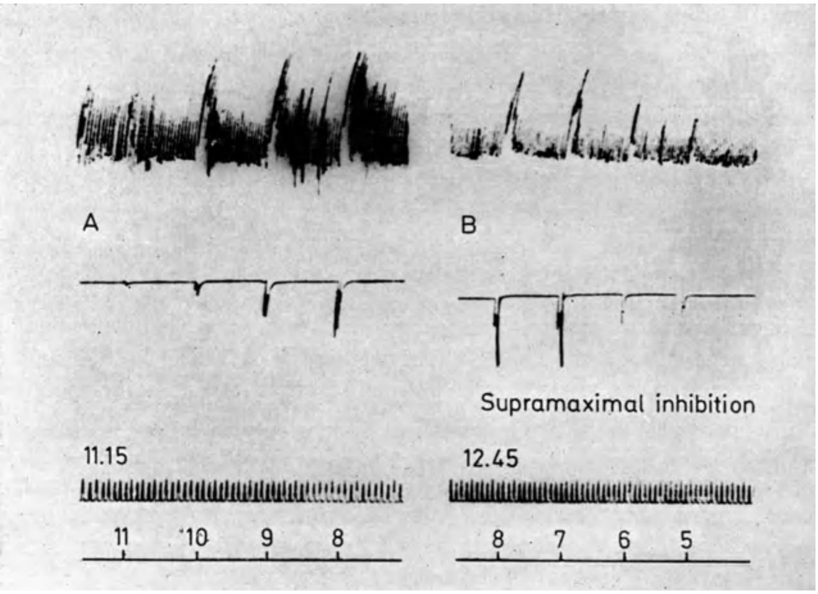

Fig. 1. Changes in respiratory and motor reactions during traumatic shock in the rabbit: A—before trauma; B—1 h 20 min after trauma.—Tracings (from above downwards): respiration; leg movements; time intervals = 2.5 sec; relative intensity of electrical stimulation (figures represent distance in cm between primary and secondary coils)

Here one is at first sight inclined to think of the type of inhibition which was termed "transmarginal" by Pavlov's school — i.e. the inhibition which protects the nervous structures from excessively strong excitation. In the light of experimental data, however, one is obliged to abandon any such assumption. The features typical of transmarginal inhibition are a weakening of vegetative and motor reactions, especially to strong stimuli, together with the preservation of the reactions to weaker stimuli. Such a pattern is seen, for example, in shock induced by mechanical trauma of the hind limbs (Fig. 1). In contrast to what occurs in traumatic shock, the dependence of the magnitude of respiratory and motor reactions on the intensity of electrical cutaneous stimulation is preserved in the hypnotic state, where a sufficiently strong stimulation leads to arousal in the rabbit and to the recovery of its habitual posture (Fig. 2).

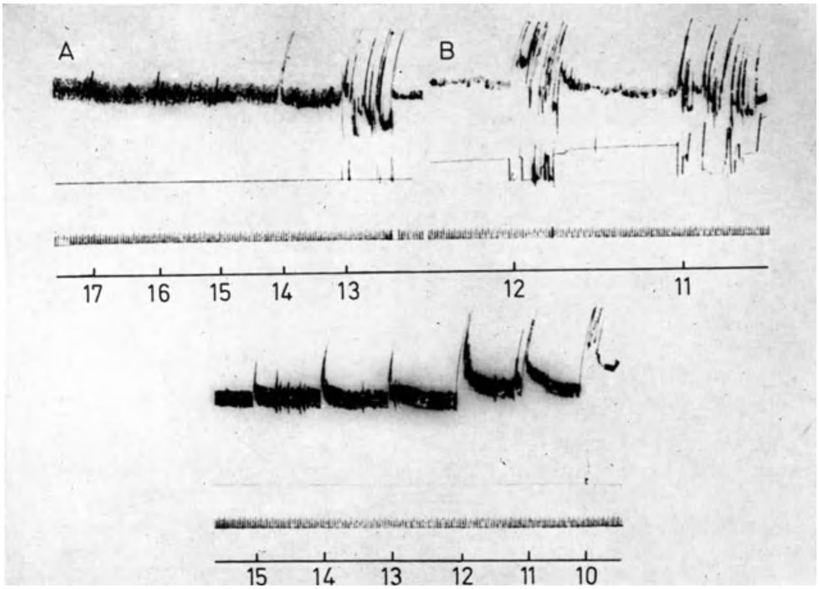

Fig. 2. Respiratory and motor reactions in the rabbit, during wakefulness (A, B) und during hypnosis (C). — (Tracings as in Fig. 1)

The assumption as to the transmarginal nature of inhibition during the hypnosis of rabbits is also in contradiction with the results of experiments in which the effects of caffeine were studied. Research by Pavlov's school has proved that caffeine intensifies transmarginal inhibition by lowering the threshold of resistance of nervous structures. The property of prolonging the duration of hypnosis is, however, possessed only by extremely small doses of caffeine; such minimal doses, which strengthen so-called internal inhibition, facilitate the extinction of non-reinforced conditioned reflexes and improve differentiation of conditioned signals. On the other hand, a larger dose of caffeine sodium benzoate has the reverse effect, being accompanied by a very considerable shortening of the duration of the animal's hypnosis (Fig. 3). The duration of hypnosis in rabbits is also shortened by generalized irradiation with X-rays. Thus, in the light of these experimentally established facts, we are compelled to abandon the concept of the transmarginal nature of hypnotic inhibition.

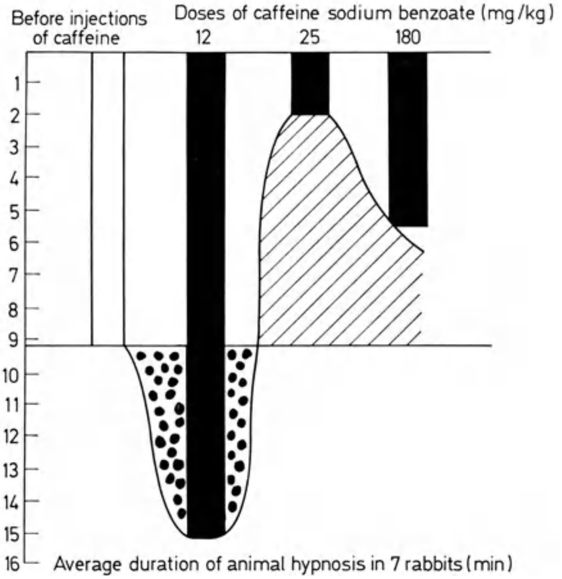

Fig. 3. Average duration of hypnosis in 7 rabbits (in min), before (white column) and after injection of 12, 25 and 180 mg/kg caffeine sodium benzoate (black columns)

At the same time, the experiments with caffeine demonstrated a definite similarity between hypnotic inhibition and that which occurs when conditioned reflexes are not reinforced. In our opinion, there are ample grounds to support this view. In fact, the most typical aspect of the procedure of hypnotizing animals is their unsuccessful attempts to free themselves and to recover their normal posture. A certain number of such unsuccessful attempts are necessary in order to ascertain that hypnotic inhibition has become sufficiently deep. Fig. 4 clearly illustrates the relation of the average duration of hypnosis in five rabbits to the time that they were fixed in a supine position. In most cases, one rotation of the body, however abruptly and swiftly made, did not ensure hypnosis unless the animal was maintained in the supine position after rotation. In our experiments, after 6, 7 and 10 (and even as many as 35) successive rotations, in every case the rabbits immediately recovered the normal posture.

Traces of the inhibition which occurs during the hypnotization of an animal in some way remain in the central nervous system, so that repeated hypnotization contributes to a prolonged hypnotic state. The continuous lines in Fig. 5 are an indication of the increased duration of hypnosis in four rabbits, following their repeated hypnotization both on one particular day, and in the course of the subsequent experimental days. The results of

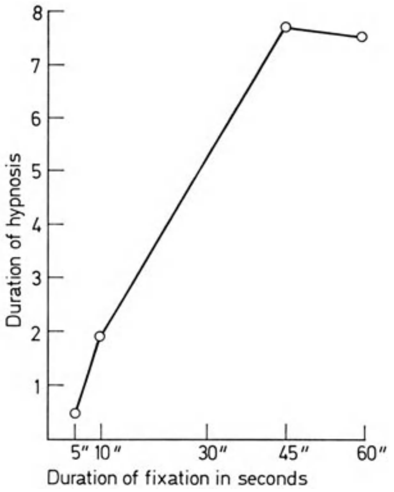

Fig. 4. Relation of average duration of hypnosis in 5 rabbits (in min) to time of fixation in the supine position (in sec)

our experiments agree with the findings of Liberson (1948; Liberson *et al.*, 1961), who prolonged hypnosis in guinea pigs by repeated immobilization with deliberate waking after each test. The fact that hypnotic inhibition is susceptible of modification again points to its similarity to extinctive inhibition of conditioned reflexes.

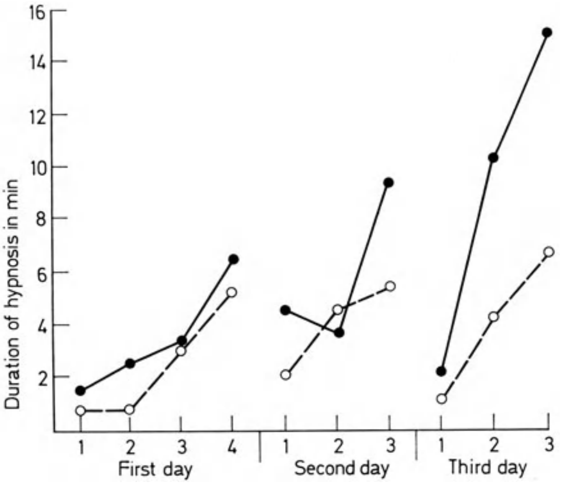

Fig. 5. Prolongation of duration of hypnosis in rabbits (in min) by repeated immobilization, in control (continuous line) and decerebrate (interrupted line) animals

Which cerebral structures are responsible for the production of inhibition during the hypnotization of animals by forcible immobilization? We became convinced, as did other investigators (TONKIKH, 1938), that hypnosis could be induced in decorticate animals incapable of independent food-procuring activity. Admittedly, hypnosis in decorticate rabbits differs to some extent from that in intact animals. In the first place, the average duration of hypnosis after decortication is considerably shortened. Secondly, if it is prolonged through repeated application during one particular experimental day, it does not subsequently exhibit any tendency towards an increased duration in the course of several consecutive days (broken line in Fig. 5). The "negative experience" acquired by the decorticate animal proves to be very unstable and soon disappears.

Although we are not justified in denying the participation of cortical structures in the production of hypnotic inhibition, the decisive role in this process appears to be played by subcortical formations: the midline thalamic nuclei (KLEMM, 1965), as well as by the reticular system of the medulla and pons (SVORAD, 1957 a, b; KLEMM, 1965; BUSER *et al.*, 1967). BUSER *et al.* (1967) presented experimental proof of the existence of a reflex mechanism in the area of the pons which inhibits the reaction of flight when the rabbit is laid on its back. According to KLEMM (1965), a slight prolonging effect is produced by direct electrical stimulation of the caudate nucleus. Stimulation of the amygdaloid complex and the hippocampus is ineffective.

In our experiments we recorded in non-anaesthetized rabbits the electrical activity of the motor area of the cortex, the hippocampus, the red nucleus, the lateral and ventromedial regions of the hypothalamus, and the reticular formation of the tectum (colliculi), as well as the electrocardiogram and the electromyogram of the muscles of the neck and the extremities. The electrical activity of the cerebral formations was recorded by means of preliminarily implanted paired steel electrodes of some 80 microns in diameter with a bipolar lead.

These experiments demonstrated that, at the period of fixing the rabbit in the supine position and immediately after freeing it, distinct desynchronization is observed in the cortex, accompanied by a sharply accelerated heart rate, also a regular rhythm with a frequency of four to seven cycles per second in the area of the red nucleus, the ventromedial hypothalamus and the reticular formation of the tectum (Fig. 6). This typical rhythm is intensified and becomes regular in the area of the red nucleus earlier than in the other subcortical formations (Fig. 7). On the other hand, it is not pronounced in the hippocampus.

In agreement with the findings of other investigators, we observed six to ten minutes after freeing the rabbit the second phase of hypnosis, indicated by slow high-amplitude irregular waves with a frequency of some

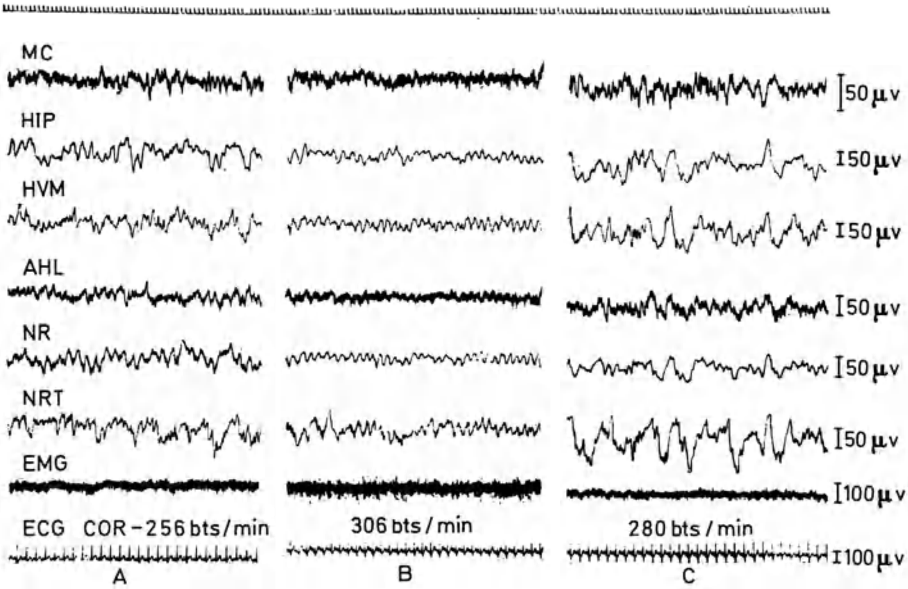

Fig. 6. EEG, EMG and ECG of rabbit during wakefulness (A), during fixation (B), and 2 min after releasing from fixation (C).—Tracings (from above downwards): EEG of motor cortex, hippocampus, ventromedial region of hypothalamus, lateral region of hypothalamus, red nucleus, and reticular formation of tectum; EMG of neck muscles; and ECG (pulse: A, 256/min; B, 306/min; C, 280/min)

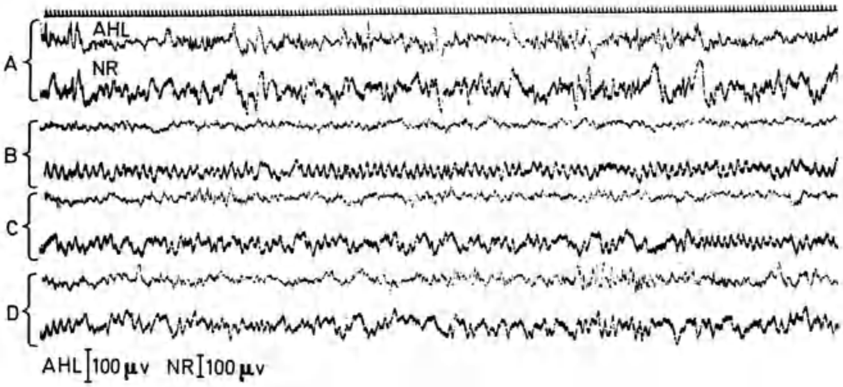

Fig. 7. EEG of lateral region of hypothalamus, and red nucleus (upper and lower recordings, respectively) in the rabbit: during wakefulness (A, pulse 159/min), during fixation (B, pulse 306/min), and immediately after releasing (C, pulse 294/min)

three to five cycles per second in all the subcortical leads and, less evidently, in the cortex. There was then a decrease in the heart rate, although it still exceeded the initial rate.

The behavioural symptoms of arousal are preceded by a pattern of desynchronization in the cortex and by the appearance of a theta rhythm in the subcortical leads. The theta rhythm appears in the hippocampus earlier than in other parts of the brain (Fig. 8). It is noteworthy that there was no well-defined theta activity in the hippocampus when the rabbit was laid on its back. Simultaneously with the electroencephalographic pattern preceding arousal, the heart rate is accelerated and the electrical muscular activity is intensified, although visible motions are not yet apparent.

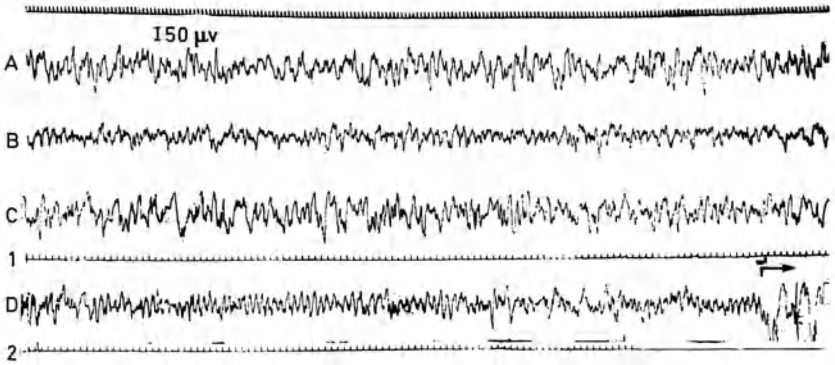

Fig. 8. EEG of hippocampus in the rabbit: during wakefulness (A, pulse 255/min), during fixation (B, pulse 356/min), 1 min after releasing (C, pulse 334/min), and before awakening (D, pulse 348/min). — Tracings 1 and 2 = flashes of light. The arrow shows the moment of awakening

The graphs in Fig. 9 show the changes in the average preponderance of delta and theta activity, respectively, in the area of the red nucleus and in the hippocampus, at different stages of the animal's hypnosis. The graphs have been plotted from the data provided by a band frequency analyser with a ten-second period of accumulation.

We are consequently in a position to map out the following pattern of events occurring in the brain when the animal is hypnotized. When the rabbit is turned over on its back, strong impulses arise from the otolithic apparatus and from the proprioceptors and cutaneous receptors (and in dogs, also from the retina). These impulses initiate the righting reflexes. The animal's unsuccessful attempts to free itself and return to the normal position give rise to a strong emotional excitation, with its characteristic vegetative shifts and electroencephalographic activity. Soon, however, there occurs a critical moment when an inhibition, which selectively

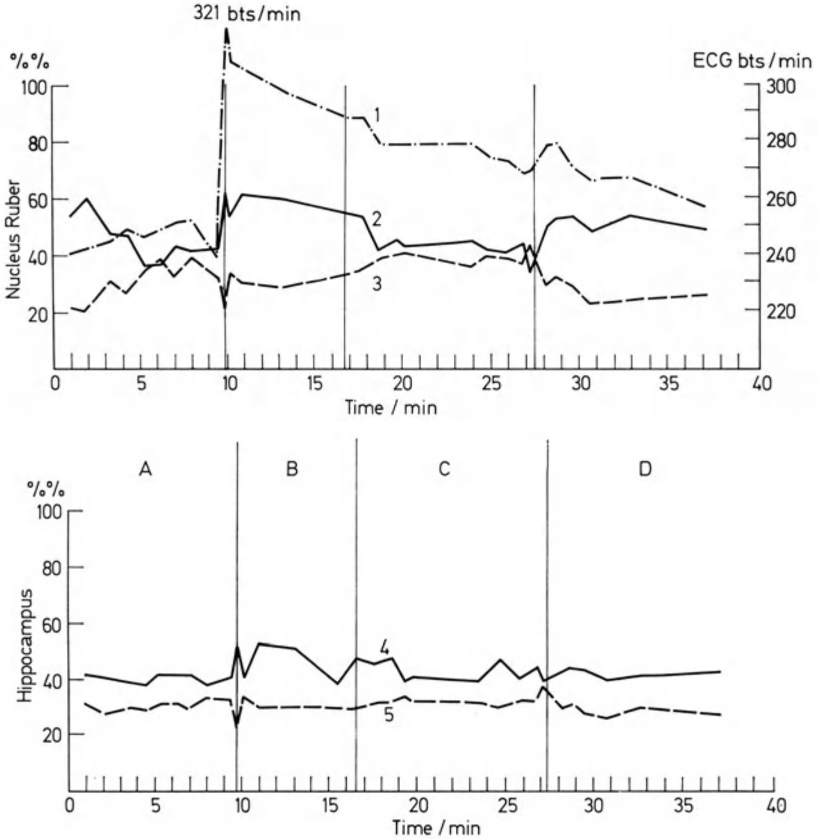

Fig. 9. Changes in average preponderance of EEG rhythms in the red nucleus (above) and the hippocampus (below), in the rabbit: during wakefulness (A), during fixation (B), after releasing (C), and after awakening (D).—1 = heart rate per min; 2 and 4 = θ-rhythm; 3 and 5 = δ-rhythm

eliminates the motor component of the emotional reaction, develops in the central apparatus of the righting reflexes (viz., their cortical representation, the ventral part of the midbrain, and the red nuclei). Excitation still predominates in the brain at this stage; there is a sharp acceleration of the heart and respiration rates, and an arousal reaction is recorded in almost every lead of the electroencephalogram.

Inhibition of motor activity in turn leads to a lessening of emotional stress. It is quite probable that this results from the elimination of the positive excitatory feed-back: proprioceptors—posterior hypothalamus—muscular tension (GELLHORN, 1964), together with the presence of positive inhibitory feed-backs, as suggested by BUSER *et al.* (1967). Under these new conditions, the predominant place is held by the deactivating systems of the non-specific thalamus and the medulla. The second phase of hypnosis, which resembles sleep, now begins, with decreased respiration and heart rates and slow wave activity in the electroencephalogram (KLEMM, 1966; and our own experiments). Thus, the hypnotic process in the animal is not one of progressive quiescence or of "falling asleep", but the active suppression of the righting reflexes in the presence of a strong generalized excitation. The synchronizing systems of the brain are involved in this process secondarily, when the elimination of motor activity provides favourable conditions for their functional predominance.

As to the animal's arousal, it would seem that the hippocampus plays an important part in this respect. The change in the electrical activity of the hippocampus proves to be the most significant indication of the impending arousal and recovery of the normal posture. It may be noted that the animal can be aroused by additional impulses from the receptors which participated in the generation of inhibition, e.g. by a slight change in the posture of the hypnotized rabbit. Such impulses serve as a signal that a possibility has arisen of recovering the normal position of the body in space, while the hippocampus appears to be a specialized detector of changes in the environment.

We believe that the hypnotic process in man is, in its more general and basic aspects, similar to that outlined above. We should abandon the concept of hypnosis as some kind of intermediate state between wakefulness and sleep. The hypnotic techniques purporting to assist the induction of sleep—such as stroking the skin, monotonous speech, fixing the gaze on a bright etc.—are of secondary importance and play no decisive role in the genesis of the hypnotic state. So-called hypnotic sleep is induced by means of specific instructions given by the hypnotist, and is a particular instance of implementing a verbal order. The essential problem in human hypnosis is to overcome the subject's active resistance, either to the process of hypnotization or to abandoning such resistance voluntarily. The difference between hypnosis in animals and man consists primarily in the fact that in man far more complex forms of resistance require to be overcome, such as a critical attitude to the hypnotist's words, or the sense of independence and of independent judgement and actions, as compared with the animal's attempts to correct an abnormal posture. Hypnosis provides a vivid illustration of imitative behaviour which proves to be preferable for the subject who lacks confidence in his capabilities and is unable to solve vital problems independently.

In an individual who has no particular reason to resist hypnosis and does not fear the procedure of hypnotization, the onset of hypnosis assumes a distinct positive emotional colouring. To most subjects hypnosis is agreeable. There is nothing surprising in this fact. According to our formula: $E = -N\ (I_n - I_a)$, the emotion changes its sign and becomes positive, with I_a greater than I_n. Indeed, in overcoming his difficulties and solving his problems by transferring responsibility to the hypnotist and placing absolute trust in him, the subject passes from a lack of pragmatic information to a situation of excessive information. It is always easier and more agreeable to assume a passive role than to acknowledge one's responsibilities and search for answers to the difficult questions presented by reality. The positive emotional colouring of hypnosis helps to implement the order to fall asleep, because positive emotions are closely connected with the activity of the parasympathetic part of the vegetative nervous system and the synchronizing structures of the brain.

Thus, the induction of a state resembling sleep is not the initial cause but one of the secondary consequences of hypnotization. The neurophysiological basis of hypnosis consists in the activation of the mechanisms of imitative behaviour developed in the course of the lengthy evolution of gregarious vertebrates. In contrast to what occurs in those animals which reproduce the behaviour pattern of the other members of the group, the imitative reflexes of the hypnotized human subject chiefly ensure a non-critical compliance with the hypnotist's verbal orders.

It is well known that under hypnosis verbal signals acquire an effectiveness such as is not encountered in the normal waking state. This is objectively expressed in shifts of the activity of the vegetative system, and in the electrical activity of the brain. It is our opinion that the phenomenon of transformation (in impersonation) of actors belonging to K. S. STANISLAVSKY's school may serve as a suitable model for studying the mechanisms of the influence of speech signals on the vegetative functions of the organism (SIMONOV, 1962).

The investigations referred to below were carried out by us, in collaboration with M. N. VALUYEVA and M. V. FROLOV, and with the constant assistance of the producer, P. M. YERSHOV. Those taking part in the experiments comprised the students of the theatrical studio and professional actors of the Sovremennik Theatre.

It became clear at the outset that the decisive prerequisite for obtaining strong vegetative shifts consists in animating the nervous traces of those direct (visual, auditory, tactile etc.) impressions which are denoted by the speech stimulus. If the subject is told to listen to a given instruction (for example: "You are jumping into cold water") and to imagine this event one minute later, it is found that strong fluctuations of electrical cutaneous potentials are recorded, not at the moment of perception of the verbal signals, but only when the sensory impressions corresponding to the given instructions are reproduced (Fig. 10).

This procedure was repeated up to forty times in the course of the experiments, all the instructions differing in content. It appeared that the

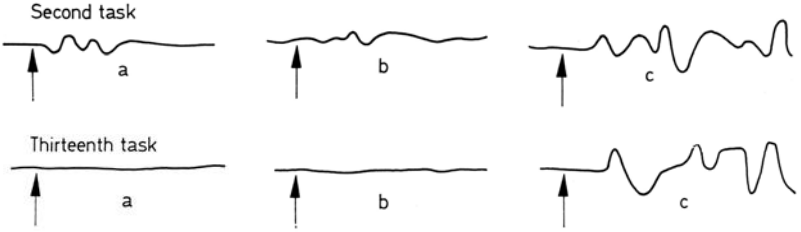

Fig. 10. Galvanic skin reflex in subject A. K.: (a) on verbal description of event by experimenter; (b) when event became detailed and specific in subject's imagination; and (c) when event was visualized in his imagination

cutaneous galvanic reaction to the perception of verbal signals becomes extinguished comparatively rapidly, while the imaginary conception of the event is accompanied, as before, by strong cutaneous galvanic shifts. We consider the disappearance of responses to verbal signals as associated with the formation of a standard scheme (or stereotype, in PAVLOV's terminology) of the perceptive act. The novelty of the signals themselves (not a single instruction was repeated in our experiments) is of no substantial importance, in this case, for disinhibiting the cutaneous galvanic reflex.

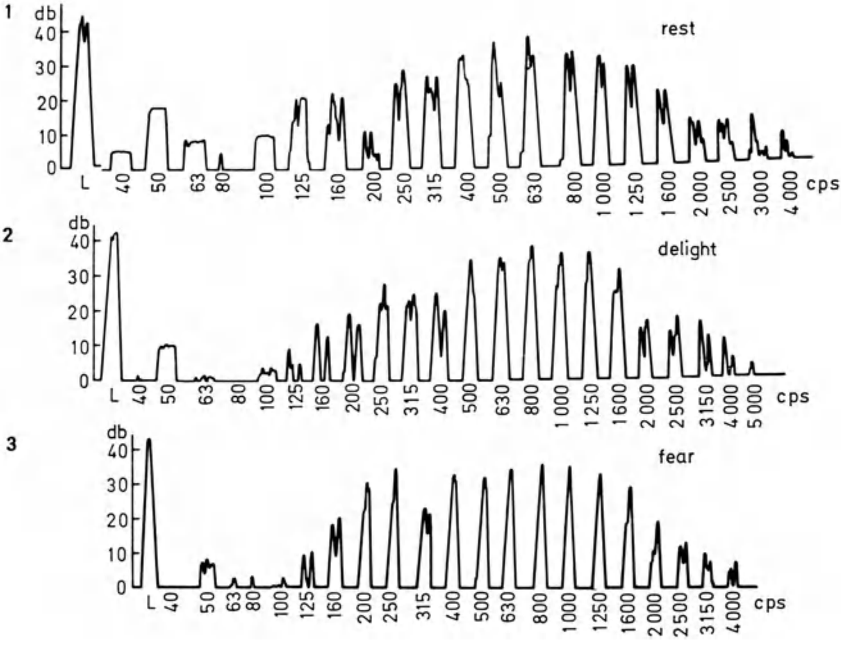

Fig. 11. Frequency spectrum of phrase: "I am Almaz", spoken by the actor V. SALUK in situation: (1) of rest, (2) of delight, and (3) of fear. — (Amplitude = db; frequency = cps)

Interesting results were obtained in studying the intonational colouring of speech. The actors were asked to pronounce certain words under various mentally reproducible conditions. For instance the actor, in one particular case, played the part of a pilot talking over the radio with the "ground" during a successful flight or in an emergency situation. It was found that the emotional stress altered considerably the intonational characteristics of the

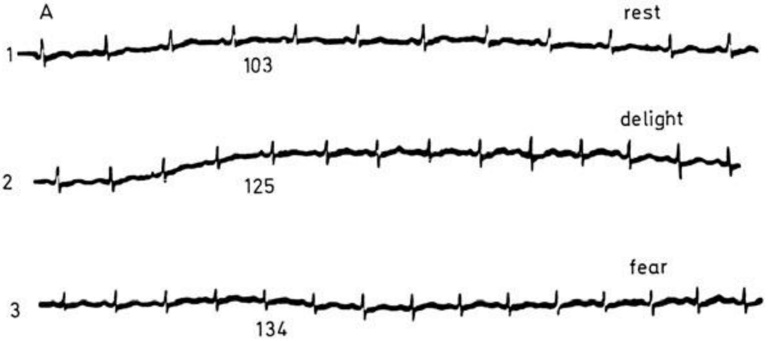

Fig. 12. ECG of the actor V. Saluk in situation: (1) of relative rest (pulse 103/min), (2) of delight (125/min), and (3) of fear (134/min)

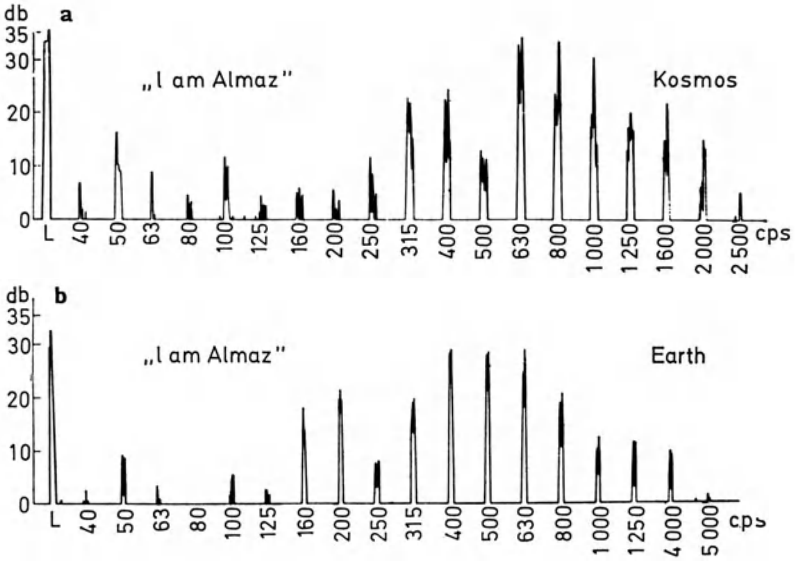

Fig. 13. Frequency spectrum of phrase: "I am Almaz", spoken by Cosmonaut A. Leonov, outside the spacecraft in outer space (above), and in a corresponding stage of training in the earth's atmosphere (below). — (Amplitude = db; frequency = cps)

spoken words, as demonstrated by means of spectral analysis of a tape recording (Fig. 11). The changes in the heart rate, recorded in the actor when he was carrying out these tasks, confirm that he was actually reproducing an emotionally coloured situation and was not imitating intonations previously observed in other people (Fig. 12). A comparison of the data so obtained with the results of analysing the same individual's speech under normal conditions* has shown that the mental conception of the event enables the actor to reproduce accurately enough the emotional colouring of speech.

The question arises as to how completely the actor succeeds in transforming himself into the person he portrays. Are there any differences between the vegetative and electroencephalographic manifestations of natural and deliberately reproduced emotions, respectively? To answer these questions, we devised a special series of experiments where a comparison was made, in the same subject, between the anticipation of real pain stimulation (natural fear) and the mental reproduction of a similar situation. In the latter case the actor definitely knew that there would be no pain. Fig. 14 shows mean values of the change in the heart rate in the last 18 sec preceding the moment of the painful stimulus, and in the first 6 sec following it. It has been established that, in the case of the actor's fear, the heart rate at first rises and then falls. In the last few seconds, when a real stimulus is anticipated, there is a tendency towards a decrease in the heart rate, which immediately after the moment of the expected stimulus (we never switched on the current) is replaced by a sharp rise.

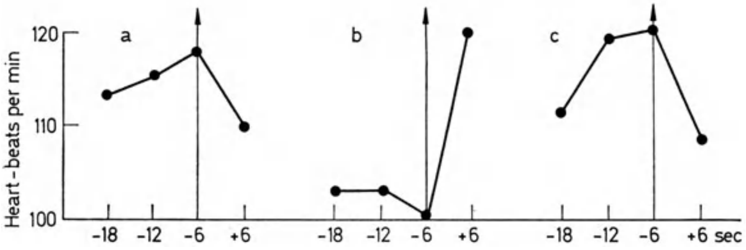

Fig. 14. Changes in average heart rate per min in 10 actors, during 18 sec before, and 6 sec after, mentally imagined (a, c) and real expected (b) electric shock. The arrows show the moment of "shock"

Analysis of the EEG frequency spectrum in the last forty seconds before the moment of the stimulation (Fig. 15) likewise revealed differences between

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* In a similar way, for example, the difference was recorded in Cosmonaut A. LEONOV's voice in the normal environment of the earth's atmosphere and when he was floating outside the spacecraft in outer space (Fig. 13).

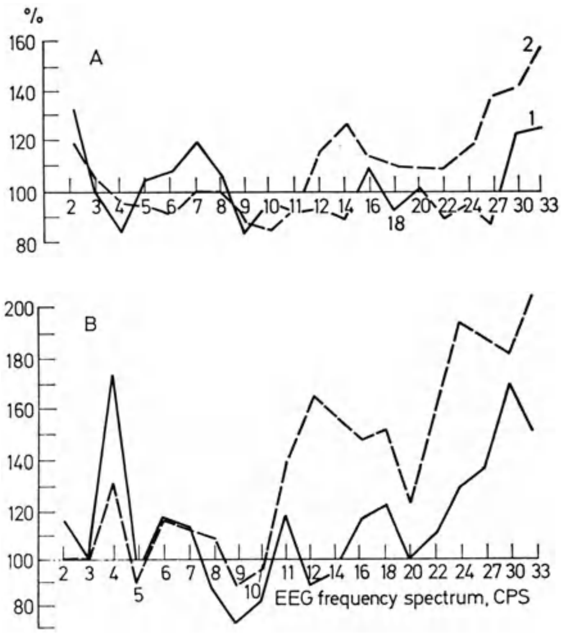

Fig. 15. Changes in mean frequency spectrum in 8 actors, expressed as percentages of the average base-rate (amplitude) at outset—which is taken as 100%: during real (continuous line) and mentally imagined (interrupted line) expectation of pain. —A—frontal lead; B—occipital lead

the manifestations of (imitated) fear on the part of the actor and natural fear. In the case of the actor's fear, there is an increase in the intensity of beta-wave activity. In natural fear, there is typically a simultaneous intensification of delta and theta activity.

Thus, these experiments have shown that in fact there is no complete transformation into the state of the person to be portrayed. The actor always retains a critical attitude to the mentally reproduced situation, and never identifies imaginary situations with reality.

According to Stanislavsky, this observation permits our regarding stage emotions as a state similar to neurosis and hypnotic trance. Indeed, both the neurotic and the hypnotized subject retain higher control over their actions. In man, it is not possible to induce by suggestion actions that are contrary to the subject's social and ethical principles. The hypnotist's orders are obeyed only within certain limits. For this reason we are inclined to subscribe to the point of view expressed by White (1947), who regarded subjects under hypnosis as, in a certain sense, actors playing the part suggested by the hypnotist.

Generally speaking, we conceive the mechanisms of hypnotic suggestion as follows. The person disposed to imitative behaviour yields most readily to hypnotization. The presence of difficulties and conflicts contributes to the activation of imitative reflexes, as one of the consequences of the involvement of the nervous apparatus of emotions. Excitation of the centres of imitative behaviour reciprocally inhibits the other forms of the subject's activity, and the hypnotist's order to imitate sleep causes the inhibition to become deeper and more generalized. The development of inhibition is also assisted to a considerable degree by the positive emotional colouring of the hypnotic state, and by the activation of the parasympathetic part of the vegetative nervous system and the synchronizing structures of the brain.

The inhibitory process plays a major part in the mechanism of deabstracting speech signals. The Soviet physiologist, KOLTSOVA (1967), has found that, underlying the generalizing function of a given word, there is the closing of a number of conditioned connections to a single, specific speech signal. For example, the word "mummy" gradually acquires for the child the meaning, not only of his or her mother, but also the father's mother, the grandmother, as well as the cat with its kittens, etc. KOLTSOVA has shown experimentally that inhibition of a considerable part of conditioned connections in turn transforms the word into a signal of a single concrete phenomenon. It may be assumed that, in the presence of a hypnotic state, the speech signal most readily animates the sensory direct traces of the phenomenon designated by the given word. This accounts precisely for those pronounced vegetative and electroencephalographic shifts which are so typical of the action of verbal stimuli during hypnotic suggestion.

Enhanced suggestibility of a hypnotized subject does not, however, imply a complete loss of critical attitude to his state. He does not identify the hypnotist's verbal signals with real facts. Like the actor, the hypnotized subject does not believe what the hypnotist tells him, but behaves like someone who *had been* in a particular situation described by the hypnotist. STANISLAVSKY figuratively called this state "magic if . . .". The hypnotized subject's higher control over his behaviour may to some extent be compared with the readiness to arousal which is always preserved by the hypnotized animal. It should also be borne in mind that the positive emotional colouring of hypnosis induces man to persevere in retaining the above-mentioned state.

A difficult situation—animation of imitative reflexes—the deepening of inhibition and, as a consequence of the inhibitory state, deabstracting of speech signals and their approximation to sensory direct traces—such is, in our opinion, the succession of events during the hypnotization of man. The nervous mechanism of emotions actually participates in every stage of the process. It is no mere chance that nearly all the investigators dealing with hypnosis noted the combination of enhanced emotionality with pronounced suggestibility and a susceptibility to hypnotization.

It is our opinion that hypnosis is a special form of wakefulness, rather than a variety of the state of sleep. This view is supported by the following facts:

1. The EEG of hypnotized subjects has much in common with that of wakefulness, and differs substantially from the EEG of natural sleep (DIAMANT, 1959; NEVSKY, 1960; MARENINA, 1960, 1961; BLANC, 1962; etc.).
2. The dynamics of responses to verbal suggestions in hypnosis reproduces all the principal regular features of conditioned activity and, notably, the reflex to time (KOROTKIN and SUSLOVA, 1963) and the phenomenon of extinction of the response to a verbal signal when repeatedly applied (LINETSKY, 1961, 1964).
3. Awake persons with a developed capacity for vivid visual conceptions can display nearly all the phenomena observed in hypnotic suggestion (BARBER, 1962; BARBER and HAHN, 1962; DAMASER, SHOR and ORNE, 1963; KOROTKIN, 1964; BARBER and CALVERLEY, 1964; VALUYEVA, 1967).

The similarity between hypnosis and some forms of neurotic disorder was already noted by neuropsychiatrists long ago. We have discussed above the common features of hypnosis and the state of stage emotion as conceived by STANISLAVSKY. The importance of studying the psychophysiological mechanisms of hypnosis extends far beyond the sphere of interest of clinical medicine. Such investigations are extremely important in seeking to understand the highly complex and little-known aspects of higher nervous activity in the healthy person.

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Mechanisms for Selective Habituation

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Introduction

When evaluating mechanisms of hypnosis, a distinction should be made between the particular functional state and the effect of suggestion. In animal experiments I. P. PAVLOV had shown that in the interval between sleep and wakefulness there occurred a number of states which are characterized by a modification of the normal relationship between the conditional stimulus and the conditioned reflex. These are: the narcotic state, when conditioned reflexes are diminished to all intensities of stimulus; the equilibrium state, when the responses to strong and weak stimuli are balanced; the paradoxical state, when a weak stimulus is more effective than a strong one; and the "ultraparadoxical" state when the negative stimulus becomes positive and vice versa.

PAVLOV regarded these states as hypnotic states since they were characterized by a dissociation of secretory and motor activity as well as by catalepsy, both of which were seen in the hypnosis of man. On the other hand, hypnotic states, when considered as a condition of "partial sleep", are characterized by very selective processes in the CNS.

Hypnotic states can be induced by repeated presentation of indifferent signals.

This paper will deal with the mechanism of selective habituation induced by the presentation of an indifferent stimulus.

1. Habituation at the Level of Macroresponses

Every novel stimulus evokes a complex pattern of somatic, vegetative and electroencephalographic responses (the generalized orienting response). In the course of repetition of a light stimulus, the generalized orienting response (OR) rapidly becomes highly localized in the visual system ("local OR"), which in turn becomes extinct relatively slowly during subsequent stimulation. An example of the "local OR" is the light-induced suppression of alpha waves in the occipital lobe.

The generalized orienting response becomes extinct selectively with respect to the parameters of the repeated stimulus. Each change of the parameter of the stimulus again evokes the "OR", and the magnitude is to some degree directly proportional to the difference between the given stimulus and a "neuronal model of the signal" set up during the initial presentation of the stimulus (Voronin and Sokolov, 1960). After extinction of EEG suppression in the occipital lobe due to a light stimulus of fixed intensity, colour, localization in space, duration and regularity, we can evaluate the configuration of the "neuronal model of the signal" by changing the parameters of the stimulus.

Polygraphic recordings of EEG (occipital, parietal and motor cortex), SGR (Tarchanoff effect), EMG (extensors of fingers) and EOG (eye movement) were made during the experiments. After 40 min of dark adaptation in a light- and sound-proof room, the level of light sensitivity was objectively measured by means of the EEG and the SGR. After a stable visual threshold was reached, 30—40 presentations of a light stimulus at 30 times threshold intensity were given. If habituation of the occipital alpha-rhythm suppression was attained, one of the stimulus's parameters was changed. The response to a novel stimulus was an indication of the ability of the CNS to differentiate the standard stimulus from the test signal. After the single test, the standard stimulus was again used to prevent dehabituation. When habituation was again established, another parameter was tested in a similar fashion.

It was shown that the diminution and the disappearance of the response were not due to a decrease in excitability. The evidence for this is firstly the EEG which was recorded during stimulus presentation. Extinction of the "OR" took place during the preservation of the initial spectrum of the EEG as obtained by the Walter Frequency Analyzer. Secondly, the extinction was not connected with a decrease in light sensitivity, since habituation was selective for the given light intensity. Responses which were absent at the standard intensity could be evoked by decreases and increases in intensity of the test stimulus (Fig. 1). An increase in intensity produced a greater suppression of the alpha-rhythm than an equivalent decrease in light intensity. This demonstrates a direct effect of the light stimulus, apart from the effect of "matching up". A stimulus intensity close to the standard produced no effect, indicating that the standard stimulus was in fact extinguished.

Thus, habituation of the EEG effect was a result of a decrease in the mismatch of the external stimulus to the internal model, as well as an increase of a selective block of the direct influence on the "OR". The "OR" is governed by both "the law of strength" and "the law of difference".

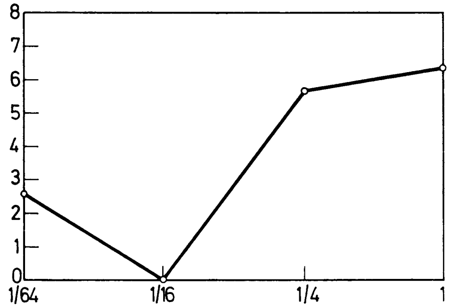

Fig. 1. Selective habituation of occipital alpha-rhythm depression to light stimulation of a given intensity in man. After habituation of the response to $^1/_{16}$ intensity using a projection perimeter, the responses were evoked by a decrease or an increase of stimulus intensity. Note that the slope is greater with an increase of stimulus intensity. Vertical axis: duration of alpha-rhythm depression in seconds. Horizontal axis: light intensity in relative units. Data averaged from three subjects

The conclusion concerning the dependence of the magnitude of the response on the change of the stimulus as well as on its intensity can be tested by changing the position of the light in the visual field. In this case the intensity was held constant and the magnitude of the response was dependent on the change of the position of the signal on the projecting perimeter. After extinction of the "OR" to a 5 mm light spot, the response could be evoked by a 10 mm spot, as well as by 3 mm and 1 mm spots. Thus, "disconcordance" stimuli can be evaluated. In the case of a smaller spot, all "energetic effects" are blocked, since the test stimulus excites the same points on the retina as did the standard.

This method of removing part of the total stimulus may be called the "subtraction method", and indicates that the "OR" can be evoked by mismatched internal signals ("disconcordance signals"). Similar results were obtained in experiments where the character of the standard stimulus was changed. Thus, a set of properties of the applied stimulus is fixed by the CNS, by a change in its elements. The "neuronal model of the stimulus" can be regarded as a multidimensional filter which selectively depresses the responses to repeated signals.

From experiments in which complex stimuli were used, it was possible to obtain details concerning the representation of single, specific properties and their connections in the "neuronal model of the stimulus".

The response to a light applied in the centre of the projecting perimeter was extinguished by repeated application of the stimulus. The subsequent addition of a tactile or auditory stimulus led to the recovery of the response, because this new combination of stimuli was not relevant to the previous "neuronal model of stimulus". After repeated application of the complex stimulus (light + sound or light + touch) habituation took place. A new neuronal model incorporated this complex, which was not equal to its algebraic sum, since the presentation of light stimulus alone again evoked the response. This means that the combination of stimuli was now fixed in the CNS.

This "subtraction method" demonstrated that even more complex signals consisting of three or more components could be fixed in this "neuronal model of stimulus".

During application of standard stimuli of constant duration and intervals, time was fixed as a parameter in the "neuronal model of stimulus". If the test stimuli were either shorter or longer than the neuronal model of the standard signal, the response started at the point in time when the test signal did not match the trace. The unexpected prolongation of the interval between the signals led to the response at the place of the omitted signal. Changing the interval of the test stimulus demonstrated that the "OR" was selectively eliminated only with respect to the interval used as the standard stimulus.

To study the localization of the "neuronal model of stimulus", KARIMOVA, BOWDEN, and SOKOLOV (1964) investigated the selective extinction of the "OR" by cutting around the auditory cortex, by the use of nembutal sleep and by bilateral extirpation of the auditory cortex in dogs. The repeated application of a standard tone (500 cps, 70 db) produced in normal dogs a gradual weakening and complete elimination of the "OR" components. The somatic component (pricking of ears) was habituated at first while local activation of the auditory cortex was the most stable component. A tone differing from the standard produced a response whose intensity and generalization increased with increasing differences between standard and test tones. Another indicator of the selectivity of the extinction was the increased probability of response to the test stimulus as compared to the standard stimulus.

In some cases the dog fell asleep, as was indicated by its pose and an increase of slow waves in several leads of the EEG. A change of frequency in the sound stimulus evoked in sleeping dogs the EEG arousal without evidence of somatic components, and the most sensitive component was the local arousal in the auditory cortex. Figure 2a, b, illustrates the lessening in degree and selectivity of habituation during sleep. In normal dogs, adding stimuli of other modalities to the standard tone, or using complex sound patterns, evoked the "OR" which was then extinguished after repetition of the new stimulus. The omission of one of the components of the multimodal stimulus usually evoked a complex "OR". After habituation to an ascending tone (250 to 1000 cps) the "OR" reappeared when a descending tone (1000 to 250 cps) was used.

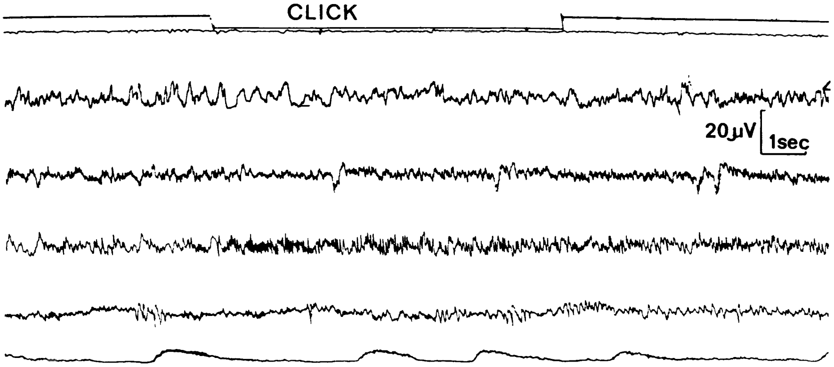

Fig. 2. Local arousal in auditory cortex of intact dog, produced by clicks after habituation to a 500 cps tone (43 min after the administration of 27 mg/kg nembutal). Reading from the top line downwards: (1) clicks at 90/min with deflection indicating duration of repetitive stimulation; (2) EMG as recorded from a muscle; EEG as recorded from (3) midbrain reticular formation, (4) thalamus, (5) auditory cortex, (6) motor cortex; (7) respiration

After extinction of the "OR" to a sequence of tones of different frequencies, a modified sequence of these tones evoked the response. The "OR" took place after the omission of a single tone given at the regular intervals, or the unexpected decrease of the interval. These data complement the results obtained in a study of the "neuronal model of stimulus" in man (SOKOLOV, 1961).

Cutting around the auditory cortex did not prevent a generalized "OR". The order of extinction of different components did not differ from that occurring in the normal dogs. Apparently, cutting around the auditory cortex did not eliminate the formation of "the neuronal model of the stimulus", which consisted of many components from various modalities. It was also evident that, after extinction of the "OR" produced by the simultaneous presentation of tone, touch and light, the omission of the tone evoked the "OR" involving a generalized EEG arousal.

A more complex situation occurred in selective habituation to the order of different sounds in a signal of successively occurring sounds. These experiments showed that the extinction obtained for a certain order of sounds was disturbed by a change in their positions in the presentation patterns. For example, habituation of the response to bells of different pitches and located in different corners of the room was selective with respect to the direction of their orderly sequence, so that a shift from a clockwise to a counter-clockwise presentation evoked the "OR" as in the non-operated dogs.

The administration of nembutal produced states of "sleep" of differing degrees. In deep sleep, no responses to stimuli were recorded. In a less deep sleep characterized by slow waves in the EEG, modified sounds produced a local arousal in the auditory cortex (Fig. 3). At a low level of sleep, a change in sound stimulus evoked the "OR" containing the EEG changes and vegetative components but not the somatic component. By comparing the results of extinction in wakefulness with that in sleep, a decrease in selectivity with respect to all EEG components was evident. In sleep, arousal occurring in the auditory cortex was the most stable component, and its complete extinction could not be demonstrated. This level of response during sleep was about 40% of the response to the standard stimulus in the wakeful animal. Selectivity of habituation during sleep could be demonstrated for both simple and complex stimuli.

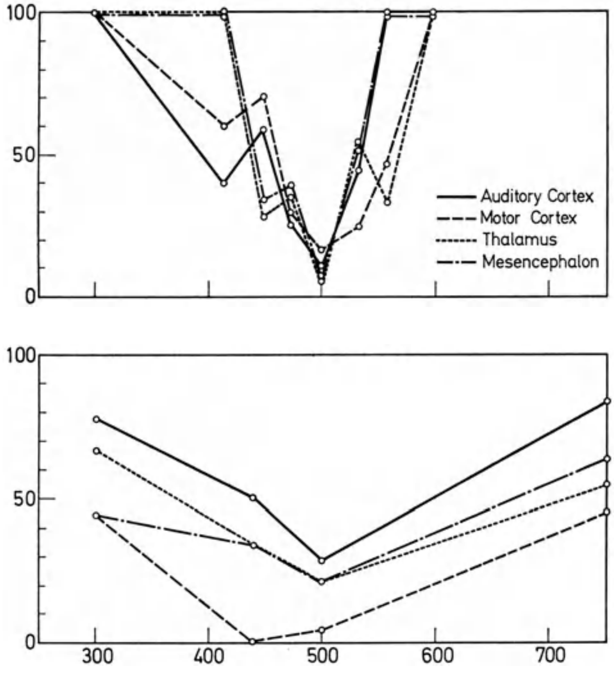

Fig. 3. Production of "OR" response by various tones after habituation to a 500 cps tone. Top graph represents response in the intact and awake dog; bottom graph represents response in the sleeping dog. Horizontal axis: frequency of tone. Vertical axis: percentage of "OR" responses obtained, using ten consecutive stimuli for each indicated tone. The selectivity of habituation seen in the awake dog is decreased in the sleeping dog, as shown by the decreased steepness of slopes from the 500 cps tone. The dominance of the response in the auditory cortex during sleep is indicated by the higher percentage of "OR" responses for each tone

Bilateral extirpation of the auditory cortex did not eliminate habituation per se, but did eliminate the selective habituation to the sequence of tones. Thus, a change in the order of successive tones did not evoke an "OR". Similar results were obtained by a modification of the order of sounding of bells located in the corners of the room.

A possible conclusion from these findings is that the auditory cortex is involved in the perception of complex signals and therefore is related to the formation of "the neuronal model" of complex sounds. The inability to perceive this complex property of the signal prevents its incorporation into "the neuronal model" and results in the lack of selective habituation of these properties of the stimulus. The persistence of the local response in the auditory cortex during sleep is evidence in support of the conclusion that a cortical mechanism for novelty response may exist.

2. Habituation Studies at the Neuronal Level in the Rabbit

A number of papers have dealt with the mechanism by which neural networks extract the properties of signals, but only a few of these studies are devoted to the problem of habituation. In order further to study neuronal mechanisms and the localization of extinction, experiments were conducted on unanaesthetized rabbits using extracellular recording of single neurons. Light flash was the most effective stimulus for altering the activity of the majority of neurons in the visual system. Even for neurons which did not respond to prolonged illumination, a brief light flash could evoke a brief response due to a transitional imbalance between excitatory and inhibitory input. A short flash of light, which evoked responses in neurons differing widely with respect to their individual characteristics, to some extent eliminated peripheral adaptation.

Four levels of light intensity were used. The interval between flashes was 2—3 sec and was greater than the transient response of most visual neurons studied. The primary experimental procedure was the repeated presentation of either light flashes or sound patterns.

Neurons in the superior colliculus, lateral geniculate body, visual cortex, hippocampus, thalamic reticular nucleus and midbrain reticular formation were found to differ in their stability of response to repeated presentation of stimulus. Of the neurons which did not modify their response to repeated presentation of stimulus, the first which should be distinguished are those that showed a modification. Neurons which were stable in their response to light flash were found in the superior colliculus, lateral geniculate body, visual cortex and non-specific reticular structures. Neurons with repetitive discharge, as studied by DUBROVINSKAIA (1966), belong to this group of stable or "non-adaptive" units. Such neurons responded to light flash with

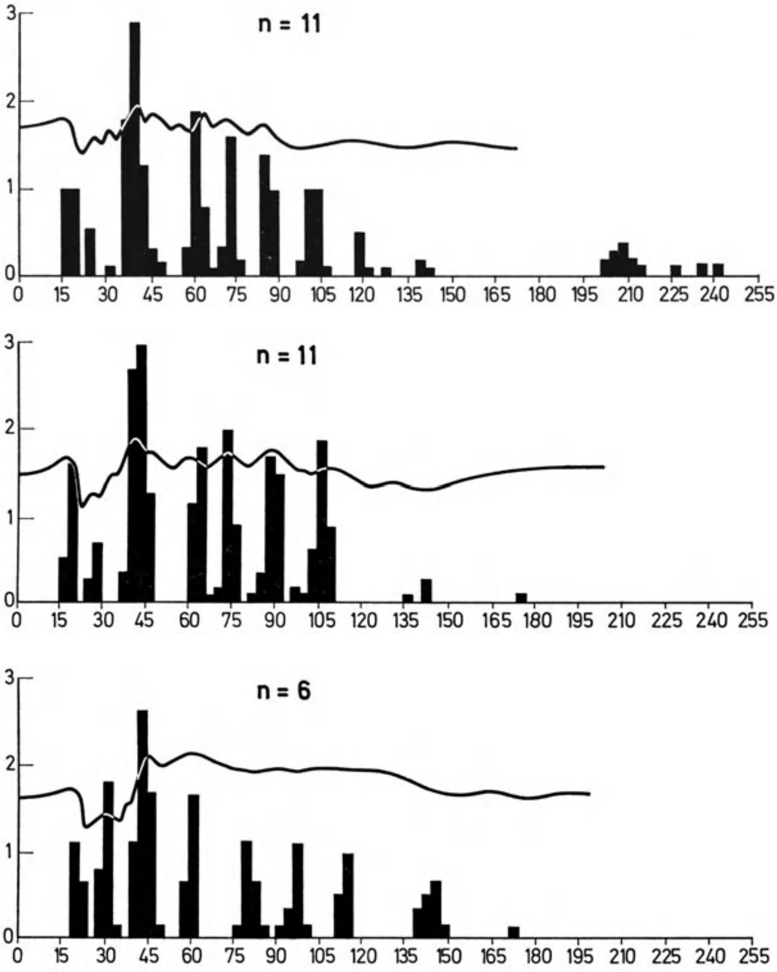

Fig. 4. Post-stimulus histograms and focal potentials of a neuron in response to different intensities of light, as recorded in the superior colliculus of the rabbit. Horizontal axis: time in msecs after presentation of brief light flash. Vertical axis: the number of spikes counted during a 3 msec interval and normalized by dividing by the number of presentations (n). The top graph represents the response at maximum stimulus intensity. The second graph represents the response in the middle range of intensities used. The bottom graph represents the response at minimal intensity. Note that the response shows rhythmicity and is in fact independent of light intensity

7—10 very dense bursts of spikes. Each burst lasted 3—20 msec with interspike intervals of 1—2 msec, and the total response lasted 150—200 msec. The response depended only very slightly on the intensity of the stimulus. A group of collicular neurons with burst discharge showed a moderate

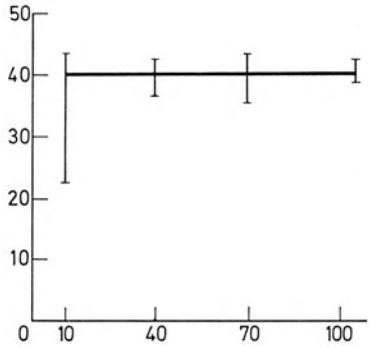

Fig. 5. Stabilization of latency of neuronal discharge in the superior colliculus of of the rabbit. Vertical axis: latency of the first spike in the neuronal response, calculated as an average from ten consecutive flashes occurring after the indicated flash number. The vertical bar for each point indicates the range. Horizontal axis: number of presented flashes before averaging the ten responses. Note decrease in range, with no change in average with increasing number of flashes

modification of their response during repeated stimulation. This modification could be characterized as a "stabilization" of the response, consisting in a sharpening of the post-stimulus histogram peaks. This effect is thought to be due to an increase in inhibitory and excitatory sequences of events during the generation of spike bursts, resulting in an increase in the probability of spike occurrence during a certain interval of the post-stimulus histogram. A second form of "stabilization" of the response was a decrease in fluctuation of the latency of the first spike in the first burst, even though its mean latency did not change.

During repeated stimulation, a depression of background firing occurred in some neurons resulting in an increase in signal/noise ratio. This ratio was calculated from the total number of spikes occurring during the total duration of response to the total number of spontaneously occurring spikes, during a fixed interval just prior to the next stimulus presentation.

A "stabilization" of response was observed with lateral geniculate body neurons (Beteleva, 1966). At the beginning of repetitive stimulation, spikes occurring in the short latency discharge phase showed a variable distribution. With repeated presentation, the latencies became relatively "stabilized" around a given value.

It should be emphasized that the "stabilization" described above was not influenced by the addition of a sound stimulus, and therefore cannot be explained on the same basis as the "OR" modification.

"Stabilization" was observed in the reticular thalamic nucleus with neurons showing a specificity to light stimulus. For such neurons the post-stimulus histogram demonstrated an increase in excitatory and inhibitory phases which were potentiated by repetitive stimulation (Danilova, 1966a, b). A similar effect occurred with "specific" neurons in the midbrain reticular formations.

Repeated presentation of the stimulus leads to a modification of the "neuronal net" as a result of potentiation of both excitatory and inhibitory elements. A neuronal net with lateral inhibition is the simplest explanation for the "stabilization" with increases in both excitatory and inhibitory input due to post-activation potentiation. "Stabilization" can be seen as either an increase in response of a neuron or a decrease in response of a neuron. A neural network of lateral inhibition is consistent with this view, since in this net neurons could be found which either increase or decrease their activity in response to a specific stimulus.

Let us consider a lateral geniculate body neuron which increases in response in the course of repeated stimulation. After 5 min of dark adaptation, the response of the neuron to light flash was characterized by a weak discharge. With rhythmic light stimulation (0.6 cps), initially a very small increase of spike discharge was observed. After 1 min of stimulation (at 0.6 cps) the response increased and became bimodal (up to two latency peaks). This effect was even more pronounced after 15 min continuous stimulation at 0.6 cps. Light stimulation of 15 cps or 80 cps did not produce a return of the response to the "pre-stabilization" level, but rather a further increase in response occurred. Clicks at 0.6 cps, used as a "nonspecific" stimulus, did not affect the neuron activity or the response to light flash. Thus, this potentiation was specific for light. Every flash of light leaves a trace in the neural net in the form of increased excitability which facilitates the response to the next stimulus. The shorter the interval between flashes, the stronger was the facilitation as shown by comparison of latency and number of spikes of 15 consecutive responses obtained with light flashes at 0.6, 2 and 3 cps. Light stimulation at 0.6 cps did not affect the discharge (Fig. 6a). The latency of discharge was very long and the number of spikes was minimal. During 2 cps stimulation the latency decreased after 5 flashes and the number of spikes in the discharge increased (Fig. 6b). The effect was more pronounced at 3 cps (Fig 6c). After 3 min of rhythmical light stimulation (80 cps) a return to 0.6 cps evoked the maximal density of spike discharge with minimal latencies (Fig. 6d).

Other neurons were found to show a progressive decrease in response with a similar pattern of stabilization. This diminishing of the response should be distinguished from extinction, where a decrease in response was also seen. The criterion for differentiating the two phenomena was the "disinhibition test". For extinction, a change in the stimulus or the presentation of an extra-stimulus led to the temporary elimination of the attained decrease in response. Habituation resulting from extinction was characterized by a selectivity with respect to the signal parameter.

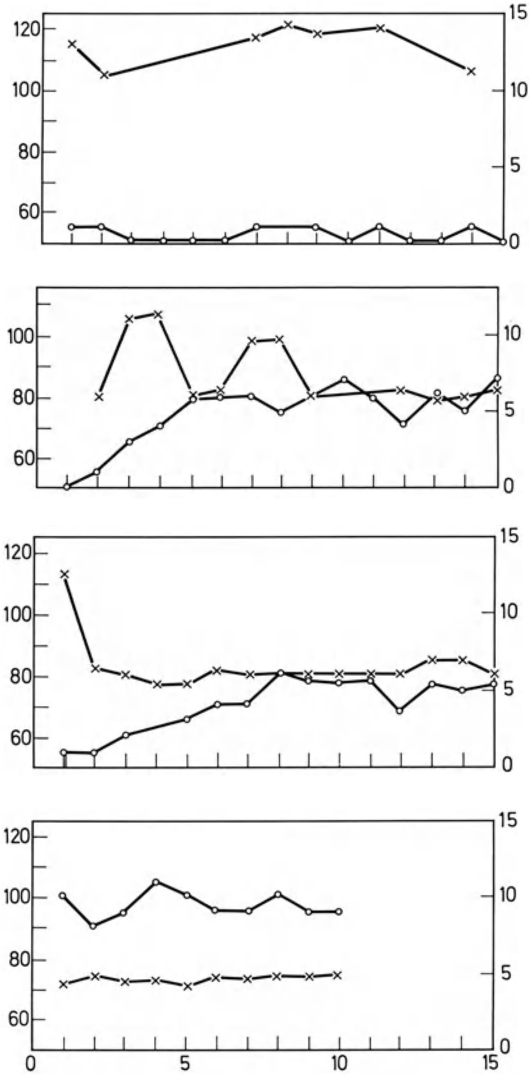

Fig. 6. Pattern of neuronal discharge in the lateral geniculate body of the rabbit in response to varying frequencies of light flash. Reading from top to bottom, the graphs represent the following frequencies of stimulation: 0.6, 2 and 3 cps; and the bottom graph represents the response to 0.6 cps after a preconditioning with light flashes at 80 cps for a period of 3 min. Horizontal axis: the order of flash sequence. Vertical axis: latency in msec of first spike in response on left, and number of spikes in discharge, on right. Curves connected by dots represent number of spikes; curves connected by crosses represent latencies. Note facilitation due to increased rates of stimulation, as well as the prolonged facilitation produced by 80 cps flashes

Neurons displaying extinction may be classified as "attention units" or "novelty detectors". Extinction consisted not only in a decrease of discharge back to the original level but also in a loss of inhibition of spontaneous activity. Inhibition of spontaneous activity was lost with repetitive presentation of the stimulus (Vinogradova and Lindsley, 1963).

Hippocampal neurons were characterized by a tonic response either as inhibition of firing (I-neurons) or as an increase in firing (E-Neurons) (Vinogradova, 1965). "Attention units" (40% of studied neurons) were predominantly multisensory neurons and responded to every change in the environment , thus possessing the characteristics of the macrolevel generalized "OR". The time course for habituation and extinction of I- and E-neurons was similar. Extinction was characterized by a disappearance of increased firing in one group of neurons and by a disappearance of decreased firing in the other group. By a systematic change of stimulus parameters (Fig. 7), the trace pattern characteristic of habituation could be determined.

Let us consider a neuron with an inhibitory response (the I-neuron). The first presentation of a one sec duration tone (800 cps) evoked inhibi-

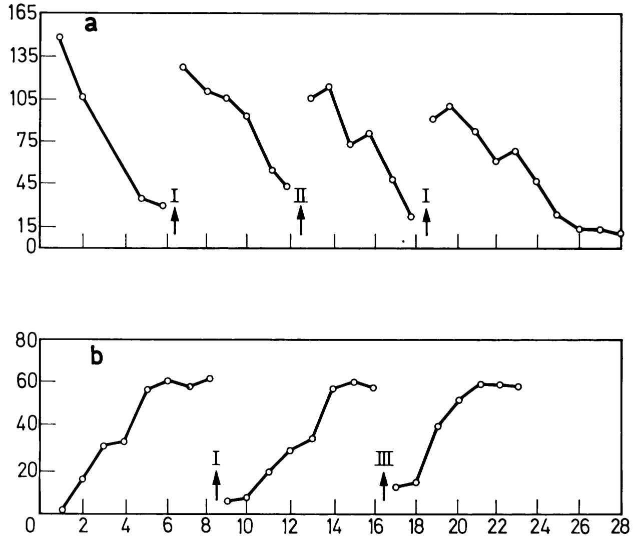

Fig. 7. Habituation of the response of an excitatory (a) and an inhibitory (b) neuron in the hippocampus of the rabbit. Horizontal axis: order of flash in sequence. Vertical axis: the number of spikes counted in a 1.25 sec interval following the flash. For the excitatory neuron, spontaneous movement of the animal (I) caused "dishabituation" of the neuron's activity. An interruption of the flash for any period of time (II) also caused "dishabituation". For the inhibitory neuron, spontaneous movement (I) and an extra-stimulus such as sound (III) produced "dishabituation". Note that in each case the "dishabituation" did not produce a complete return to prehabituation level

tion of spontaneous activity. After a number of presentations, this inhibitory effect was lost. Thereafter, a tone at 400 cps evoked the inhibitory effect. After habituation of the 400 cps response, a 300 cps tone could now evoke inhibition, which could then be habituated, and so on. In this case, a difference of 100 cps was an effective stimulus. The response of the neuron is a function of novelty.

The response to a combination of click and a 800 cps tone could be habituated, and the presentation of the 800 cps tone alone again evoked the response. The omission of a component from the complex stimulus produced the response of the neuron. This means that neurons of the hippocampus can fix the complex properties of stimuli. Thus the responses of hippocampal neurons are represented by the trace pattern or "neuronal model of the stimulus" induced by repeated stimuli.

Of special interest for an understanding of the mechanism of habituation is the observation of POLIANSKII (1966) that neurons located in the deep layers of the visual cortex showed summation of hyperpolarization. This cumulative inhibition resulted in the depression of neuronal discharge (Fig. 8).

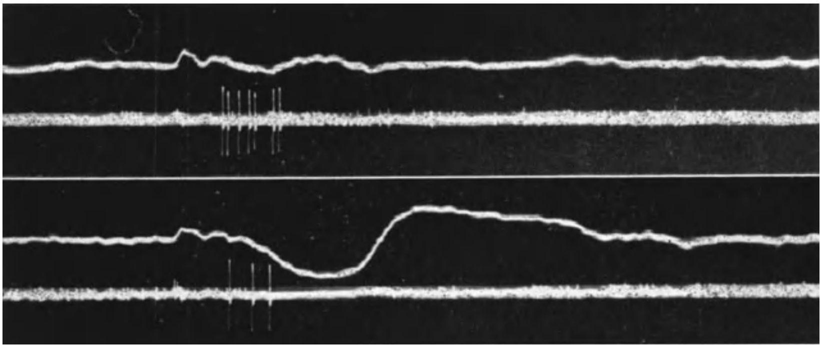

Fig. 8. Inhibition of visual cortex neuron due to repeated presentation of a light flash. Focal potential, first tracing, and neuronal discharge, second tracing, are shown for the response to preceding light flashes at the first presentation (top two tracings) and the fifth presentation (bottom two tracings). Vertical lines indicate light flash. Positivity is indicated by downward deflection of the tracing. With an increase in the positive slow wave, a corresponding decrease in neuronal discharge occurs

A reasonable hypothesis is that signals reach "attention units" in two ways: directly, in the form of EPSP's, and in parallel in the form of IPSP's via inhibitory inter-neurons. Secondly, it is suggested that a preferential potentiation of inhibitory synapses occurs in the course of stimulation. The matrix of potentiated synapses which represents the several properties of the stimulus acts as a locus of storage of the trace pattern, and thereby represents "the neuronal model of the stimulus" (Fig. 9).

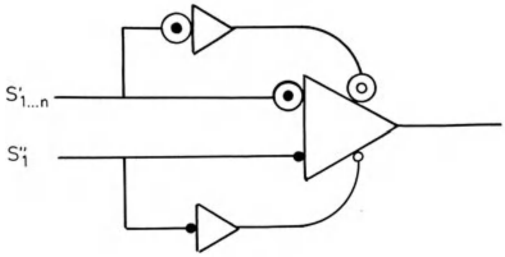

Fig. 9. Simplified schema for selective habituation. Idealized neurons are indicated by triangles, with circles representing inhibitory synapses, dots representing excitatory synapses and the additional outer ring indicating potentiated synapses. $S'_{1} \ldots _{n}$ represents the channel for the standard signal after n presentations. S''_1 represents the channel for the first test stimulus. After selective potentiation of the set of synapses representing the channel for S''_1, the output drops to zero and the cell is completely habituated to S''_1. This assumes that inhibition is potentiated at a faster rate than excitation. A novel stimulus S'' can still evoke a response since it utilizes a different set of synapses, as represented by the second channel

A group of superior collicular neurons responded to light flash by inhibition of spontaneous activity, and only the latter part of this inhibition was habituated. Let us consider one of these neurons in detail. A single flash produced inhibition of spontaneous firing for a period of 500 msec with a latency of about 30 msec. In time (presenting the light flash at 0.6 cps), the latter part of inhibition disappeared with a recurrence of normal spontaneous firing, while the initial part of inhibition was not habituated. Inhibition was usually followed by "rebound" excitation characterized by an increased spike frequency. In the process of extinction of inhibition, the "rebound" was likewise habituated. Presentation of an extra-stimulus (sound) eliminated the extinction of the latter portion of the inhibition, and the initial degree of inhibition was re-established. Simultaneously, the "rebound" was also seen again (Fig. 10). About 100 presentations were necessary to habituate the latter portion of the inhibitory phase but only six presentations were necessary to re-establish habituation blocked by the sound extra-stimulus.

In these superior collicular neurons, the inhibitory response was the result of the summation of adaptive and non-adaptive inhibitory influences arising from different levels of the CNS through independent channels. The adaptive component was preferentially habituated, and therefore the duration of inhibition became shorter. Neurons of the visual cortex also demonstrated such a partial habituation. For this type of neuron, it would appear that specific and novelty signals are summated.

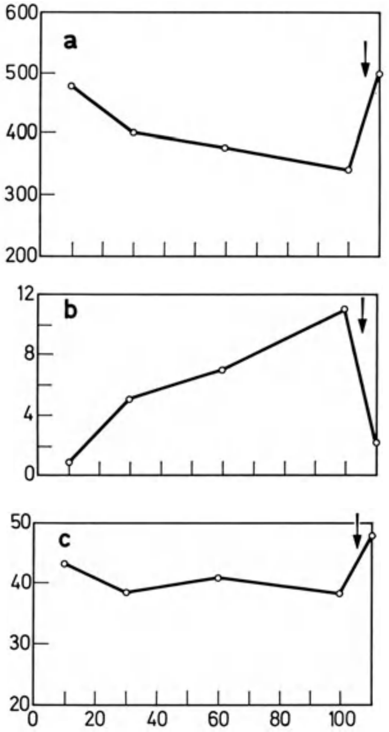

Fig. 10. Partial habituation of neuronal response in the superior colliculus of the rabbit. Horizontal axes of all three graphs: cumulative number of flashes. Vertical axes: (a) duration of inhibition of spontaneous activity in msec; (b) number of impulses occurring during the inhibitory phase; (c) number of impulses occurring in the excitatory rebound phase which takes place after inhibition. The arrows indicate the application of a sound stimulus leading to the recovery of the level of habituation reached at that time

Habituation occurred also through the thalamic reticular nuclear neurons (DANILOVA, 1966a, b). The main feature of the response of these neurons during the resting state of the animal was a discharge pattern organized in synchronized bursts of spike activity. In response to various stimuli, the synchronized bursts were replaced by a train of irregular firing of the neuron. In the course of prolonged (up to 30 sec) stimulation (using 4 clicks per sec), a disappearance of this "desynchronization" took place with the reappearance of the synchronized bursts of spike discharge. The repeated presentation of 5 sec duration stimuli produced habituation of "desynchronization", as shown by a corresponding increase in the bursts of spikes as the number of irregular spikes decreased. The test which demonstrated the relation of this effect to habituation was the "disinhibition effect". After extinction of "desynchronization", a single extra-stimulus could give rise to the temporary re-establishment of "desynchronization". This dishabituation could result either from a change of the time order of stimulus presentation, or by spontaneous motor activity on the part of the animal.

The synchronization of spike discharge in bursts may be explained in terms of negative feedback from Renshaw-like neurons. Desynchronization occurs as a result of interruption of the internal cycle by inhibitory collaterals from "novelty detectors".

3. Habituation of Mollusc Neurons

The proposed mechanisms of habituation may be evaluated by studying the relatively simple neural network of molluscs. DULENKO and SOKOLOV used tactile stimuli applied to the skin of the snail's "foot". A prolonged increase in the firing of pedal ganglionic neurons was observed in response to a single brief application of a tactile stimulus. This prolonged increase in the rate of firing can be described as a form of "arousal". The repeated presentation of the tactile stimulus to the same skin area at 1—5 min intervals resulted in a decrease or complete loss of response. Stimulation of a neighbouring point evoked a response, although smaller than the original one. Recovery of the response occurred after a period of rest.

In the course of repeated stimulation, the increased firing was sometimes followed by inhibition before the initial level of spike frequency was re-established. Habituation was also seen with silent neurons which responded to tactile stimulation.

In stimulating the skin, adaptation at the receptor could not be eliminated. Orthodromic electric stimulation of the nerve cells was used to exclude the receptor in the study of habituation. The giant neuron of *Limnaea stagnalis* was studied for this purpose.

Microelectrodes were Pyrex glass pipettes (1 μ tips) containing 2.5 M $FeCl_3$ solution, with resistances of 15—20 megohms. Histological localization of the position of the microelectrode in the neuron was made by electrophoresis of Fe ions (SOKOLOV, ARAKELOV and LEVINSON, 1966).

For one group of spontaneously active neurons, orthodromic electrical stimulation evoked a biphasic response, consisting of an initial increase followed by a decrease in the firing rate of the neuron with eventual recovery to the pre-stimulus level. The disappearance of this biphasic response during 1 cps stimulation was characteristic of these neurons. Parallel inhibition (i.e. the splitting of the incoming activity into an excitatory and a parallel inhibitory input) may be used to explain the mechanism of this habituation, and it follows that the thresholds for excitation and inhibition can be different since the two inputs are in essence independent. The presence of such neural networks may be ascertained by measurement of the thresholds for excitation and inhibition.

On decreasing the intensity of the stimulus, the excitatory phase of the response disappeared while the inhibitory phase remained. The independence of the inhibitory input was also demonstrated by the use of a repetitive weak stimulus. A sequence of 7 orthodromic stimuli evoked no excitation and produced the inhibitory phase as shown by an increase in interspike interval (Fig. 11).

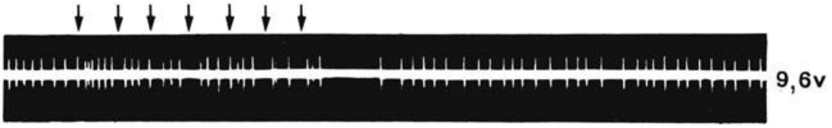

Fig. 11. Summation of habituation due to repeated near-threshold orthodromic stimulation of giant neuron in a mollusc. Arrows indicate moment of stimulus (9.6 V; 0.5 msec duration). Note the occurrence of inhibition following the seventh stimulus

A second group of spontaneously active neurons was characterized by occasional spontaneous bursts of activity similar in pattern to the biphasic action described for the first group. With the use of orthodromic electric stimulation, it could be seen that the threshold for excitation was higher than that for inhibition. The duration of the inhibitory phase was only slightly dependent on the duration of the excitatory phase. A diminution of the stimulus decreased both excitatory and inhibitory phases. Repeated orthodromic stimulation at 1 cps (16 V, 0.5 msec) led to a gradual decrease and elimination of the excitatory phase with no habituation of the inhibitory phase.

The initial response to orthodromic stimulation at 0.1 cps was similar to that at 1 cps: the burst of spikes evoked was followed by an inhibitory phase. With continued presentation of the stimulus, the number of spikes during the excitatory phase decreased. After 7—8 stimuli, not only did the excitatory response decrease, but an occasional spike occurred during the inhibitory phase. With further stimulation, the discharge decreased during the excitatory phase, and the number of spikes increased during the inhibitory phase, until no difference could be observed between background activity and activity occurring after the stimulus (Fig. 12).

Thus, extinction of the neuron's response to 0.1 cps orthodromic electrical stimulation can be divided into two stages. The first stage, where habituation of excitation occurs, can be explained by potentiation of parallel inhibition. The second stage, where both excitation and inhibition are decreased, can be explained by pre-synaptic inhibition.

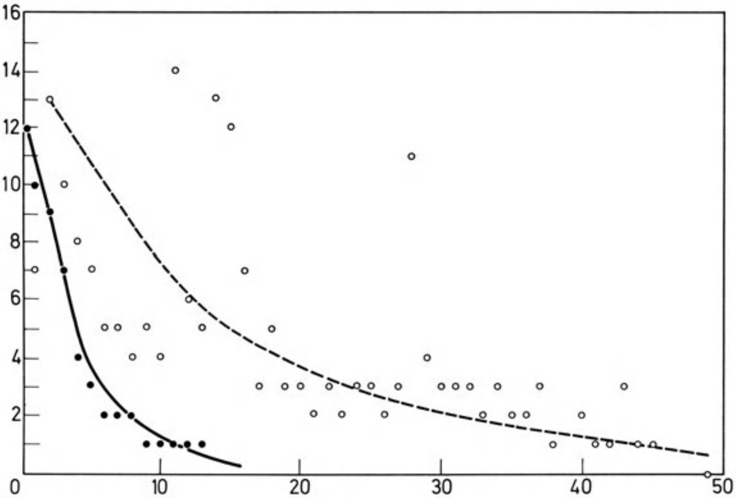

Fig. 12. Habituation of response to orthodromic stimulation of giant neuron in a mollusc. Habituation of the response occurred faster at 1 cps (filled-in circles) than at 0.1 cps (open circles). Horizontal axis: order of (16V) stimulus in sequence. Vertical axis: number of impulses occurring in the response

Summary

1. Habituation can be demonstrated at the unit level, which is similar n its characteristics to habituation at the macro-level.

2. This habituation is due to a potentiation of parallel inhibition, with a possible additional effect of pre-synaptic inhibition.

3. Selectivity of habituation is due to a matrix of potentiated synapses, which represent the trace pattern, and also the "neuronal model of the stimulus".

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The Physiology of Controls on Sensory Pathways with Special Reference to Pain

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The nature of the phenomenon of hypnosis has been described elsewhere in this symposium. In hypnotic anaesthesia, patients fail to report pain, fail to recall their experiences after surgery and fail to demonstrate those autonomic and withdrawal reflexes which are to be expected during injury to tissue. The present state of knowledge of the physiology of central nervous system mechanisms associated with pain is not sufficiently developed to offer any explanation at all for the bizarre and curious state of a hypnotized patient. This inability of physiologists is not surprising since no sensory process can be followed from stimulus to conscious experience. However, a considerable amount is known about pathways which conduct pain producing impulses from periphery to those higher structures traditionally associated with conscious experience. It is evident that in hypnosis an extraordinarily effective blockade can be established across these connections from afferent inputs to effector mechanisms. It is reasonable therefore to ask a physiologist to speculate on the possible sites of this blockade without his attempting to define the nature of the mechanisms which generate the orders which result in the setting up of the barricades.

The sensory system imagined by Aristotle, Descartes and most modern philosophers is shown in Fig. 1. The essence of the diagram is that events are detected, and then transduced and transmitted by sensory systems to some actual structure. It is assumed that the sensory systems collect to the best of their ability a true and accurate picture of the state of the body and the external world. The report of the sensory systems is then interpreted by some subsequent mechanism or "mind". This two-stage process consisting of transmission followed by interpretation, maintains a dualistic philosophy impressed in anatomical and physiological terms. In this model of sensory experience, the sentient man is enclosed in a capsule made of his own skull and is connected to the outside world and to his own body by an array of monitors which he passively observes. The external world is

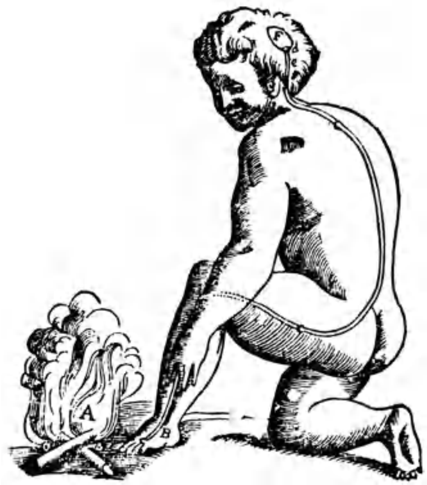

Fig. 1. Descartes' picture of the sensory system. The existence of the stimulus is communicated to the sensorium by a straight transmission system. Some afferent systems actually exist which approach this diagram in their simplicity and specificity. Others however are subject to censorship and control

imposed on the sensory receiving structures in an accurate and immutable fashion. Higher structures are responsible for the admission, rejection and perception of the meaning of the display provided by sensory systems. If this extreme model were a true picture of the sensory apparatus, then the physiologist could contribute nothing to an explanation of wide deviations of stimulus-response relations because we have mainly studied the function of transmission pathways.

In extreme contrast to this commonly accepted picture of sensory mechanisms, one could propose an alternative theory in which sensation did not result from an imposition of the world on the senses but rather that sensation followed an intentional search for information from the world and the body. If sensory experience is actively obtained by the central nervous system rather than imposed on it, then one would expect to encounter an apparatus for exploration linked to the sensory transmission systems. We see external signs of exploration going on during discrimination. These are alerting, orientation, looking, sniffing, and manual explorations. While such obvious motor movements are in progress, it would seem possible that the sensory transmission systems are themselves being set to receive the predicted class of events. Furthermore, since sensory experience is not an instantaneous report of the state of the world but follows a spatial and temporal integral of events, there is considerable time elapsing between stimulus and sensory response. During this time, it is possible for internal exploration or requestioning to take place in which the sensory signals are filtered and re-examined. Given the existence of two opposite theories, one predicting a rigid and accurate sensory transmission system and the other a highly modifiable sensory transmission system, the physiologist must examine the actual situation with experiments designed to allow the systems to demonstrate the extent of their rigidity or modifiability.

Nerve impulses generated in the axons of peripheral nerve fibres by stimulation of the skin are conducted to the spinal cord. Some of the nerve fibres on entering the spinal cord divide into two branches. One branch divides into the nearest cellular region of the dorsal horn of spinal cord. The other branches proceed towards the head and make up the major component of the dorsal columns. They terminate on the dorsal column nuclei whose axons project to the ventral posterior lateral (VPL) nucleus of the thalamus via the medial lemniscus. From this nucleus, axons project to the parietal cortex. This pathway has evolved along with the cerebral cortex and has, for very good reasons, received the major attention of physiologists. The characteristics of the pathway are that both the spatial organization and the detailed physiological specificity of the peripheral nerve fibres are retained at each stage. In the three synaptic regions, the dorsal column nuclei, thalamus and cortex, the cells are arranged in a detailed somatotopic map so that the surface of the body is represented by homunculi at each stage. Each cell has a small receptive field on the surface of the body and the stimuli which will cause the cell to fire are highly limited and specific. Certain descending fibres end on the cells of this ascending pathway. In the dorsal column nuclei, for example, some cells receive impulses from the pyramidal tract and from the reticular formation. In spite of these intrusions, the transmission characteristics of the pathway are remarkably stable. The responses of these particular thalamic cells are essentially similar in the anaesthetized and unanaesthetized monkey. In man the responses of cells in the VPL nucleus have been examined during the course of thalamotomy operations and it has been repeatedly observed that the cells maintain their characteristic discharge to stimulation of their receptive fields during the distractions of other stimuli, or during the time when the patient is concentrating on such tasks as mental arithmetic. It seems apparent that this afferent pathway is bringing to the cortex relatively uncensored information. If is therefore a very poor candidate as a site for blockade and those signs of its activity such as the major component of the evoked cortical potentials are unlikely to be useful indicators of the receipt of those signals which trigger conscious sensation.

By contrast to the dorsal column nuclei-lemniscus pathway, the fibres from dorsal roots which terminate on the cells of the spinal cord dorsal horn transmit their information to an entirely different system. The cells which seem at present to be the best candidates as the transmitters of information about painful states lie in lamina 5. This lamina of grey matter runs the entire length of the spinal cord and is situated in the middle region of dorsal horn. Small myelinated fibres from viscera, muscle and skin end directly on these cells of lamina 5. These small diametre fibres have always been implanted as important transmitters of information about tissue damage. In addition to small diameter fibres, large diameter fibres of cutaneous origin converge indirectly on to these cells and have a mixed inhibitory and excitatory effect. The overall firing pattern of the cells depends on the balance in the afferent barrage of large versus small fibres. In addition to this complex interaction of fibres in the afferent volley, impulses descending from the head have a strong influence on the cells of lamina 5. For example, in the decerebrate cat, cells in lamina 5 have relatively small excitatory cutaneous receptive fields with extensive inhibitory surrounds. The driven response of such cells following the arrival of an afferent barrage is cut short by inhibition. The response of the cells to light repeated stimulation habituates. However, if impulses descending from the brain are prevented from influencing the cells by cold blocking the descending pathways, then the size of the excitatory receptive field expands while the inhibitory surround shrinks. The driven response of the cells is followed by a period of facilitation lasting for seconds and habituation disappears. It is quite apparent that these cells are under dramatic control from the head which affects the extent of their receptive fields, the duration of their excitation and the convergence of different types of impulses on to them. In addition to control from the hind brain, we know that many of these cells are affected in a complex way by stimulation of the corticospinal pyramidal tract. Recordings have been made from cells in this region in the freely moving animal and it was obvious that continuous and radical shifts of excitability of the cells were associated with various forms of behaviour. We see then that these cells contrast with those observed in the dorsal column-medial lemniscus system in that fibres of different types converge and interact on them and in that descending pathways strongly influence their responses to afferent impulses. Axons from the cells in lamina 5 affect both local autonomic and flexor reflexes and project to the head.

We have suggested above two alternative intuitive models of sensory systems. One model would bring all available information in a continuous stream to central structures while the other model suggests that information is obtained by an active process of search and selection. On examining the actual situation in a mammal, one finds that both types of system exist. The dorsal column-medial lemniscus system is a recently evolved highly refined sensory transmission pathway of the rigid and specific type. In parallel with this system, we have described an example of the other type of afferent system in which many different types of information can be handed on and in which orders from the head control the type of information handled by the pathway. Faced with two parallel pathways organized in such radically different ways, one must try to suggest their actual roles in sensation. It is highly unlikely that they represent alternative pathways or that one has superseded the other. Even if we limit our discussion to a consideration of pain mechanisms, we can obtain some clues to a possible function for both pathways. Surgical section of the dorsal columns does not eliminate pain reactions to peripheral stimuli. Electrical stimulation of the VPL nucleus of the thalamus in Parkinsonian patients does not elicit painful sensations. On this evidence, one might be tempted to conclude that the dorsal column-medial lemniscus system has nothing to do with pain. However, if this nucleus is stimulated in patients suffering from pain then their particular pain is markedly increased.

Contrasting with this rigid system, we have described the system originating in dorsal horns which is so strongly controlled by impulses from the head. Stimuli to peripheral fibres known to end on the lamina 5 cells set off painful sensations. Section of the ventral white matter in spinal cord abolishes the ability of stimuli below the level of section to trigger pain reactions. There is therefore good evidence that this is the pathway which leads to conscious sensation but one must ask what factors and mechanisms control the descending control systems. The following hypothesis suggests itself: the rigid system brings to the higher centres uncensored information on the state of the body. On the basis of this information, higher centres issue orders to the modifiable pathways which now transmit information which triggers conscious sensation. The tactics of the higher centres are formed by their genetic and environmental background. This theoretical model of sensory mechanisms suggests for an experimental analysis of hypnotic analgesia the following possibilities: 1) the verbal orders of the hypnotist determine the set of the modifiable sensory afferent pathways; 2) one of the locations at which these orders might impress themselves is at the first central synapse in the dorsal horn of the spinal cord; 3) intrusion at this synaptic region would suppress not only the messages which trigger conscious sensation but also would suppress the local segmental reflexes; 4) hypnosis would not be expected to modify impulses in the rigid pathway, or to modify signs of the activity of this pathway such as evoked potentials on the cerebral cortex; 5) if hypnotic analgesia were produced in part by the descending control of afferent sensory pathways, it might be that with the suggestion that pain would not be felt there would be a concomitant rise of threshold of other types of sensation not mentioned in the specified suggestions of the hypnotist. The aim of this paper has been to stress that some afferent pathways are rigid while others can be censored at each stage. The modifiable pathways might be affected, even at the spinal level, during hypnosis.

II. Experimental Psychology

Some Peculiarities of Posthypnotic Source Amnesia of Information

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In recent years the problem of posthypnotic amnesia has again attracted the attention of researchers. They are studying both its nature (GRANONE, 1965; HILGARD, 1966; ORNE, 1966) and the different forms of its manifestations.

Some of the forms of posthypnotic amnesia which are now being investigated in various ways are related to so-called source amnesia. This term refers to a peculiar form of posthypnotic amnesia, characterized by the fact that the subject, though capable after the annulment of hypnosis of reactualizing the information obtained during it, nevertheless is not aware of the circumstances under which he came by this information; hence, the amnesia is manifested in relation to the source of the information, and not the information proper. This type of amnesia, observed a long time ago, but analysed and designated as such only recently (EVANS and THORN, 1966), is differentiated from that called by THORN "recall amnesia", by which he means the traditional form of posthypnotic amnesia, namely the subject's inability to remember in the waking state the events which took place during the hypnotic state. We shall call this type of amnesia the posthypnotic amnesia of information, which we differentiate from the posthypnotic amnesia of the source of information.

We have set ourselves the problem of investigating whether:

a) source amnesia represents an isolated phenomenon, or if it may be found generally in all subjects where information amnesia has been observed;

b) source amnesia presents only one, or several forms of manifestation.

Experimental Model and Results Obtained

The research was carried out during 1966 on patients of different ages and occupations, suffering from various neurotic affections. For this study, only those subjects were selected, in whom posthypnotic amnesia had previously been observed (without its having been specially suggested), and in whom hallucinations, age regression and analgesiae had been provoked by suggestion. Such subjects were found in approx. 7% of a group of hypnotizable subjects. The hypnotic state necessary for the experimental investigations was always induced by the same experimenter (who did not belong to the clinic where the research was carried out). The control group was matched to the experimental one, as regards sex, age, profession and neurotic affection.

In order to study these problems, the following procedures were applied.

I. The Reproduction and Recognition Test of Two Series of Words

The subjects (N = 31) were presented individually with a group of ten words; their task was to repeat each word exactly as it was pronounced by the experimenter. Since the effects of involuntary memory were being investigated, a special explanation was given to mask the mnesic task. The subjects were told that we were studying the way they pronounced words of different phonetic structure. After repeating the tenth word, the subject was put into an hypnotic state. About five minutes after the induction of hypnosis, the same experimenter presented in the same manner a second series of ten words. After 8—10 min, the subject was returned to the normal waking state, and was asked when, and how many times, some words had been spoken to him, and what these words were. After recording the results of reproduction, a list of 60 words was produced, which also contained the two series of ten words which had been presented before and during hypnosis, and the subject was set the task of indicating the words known to him.

This test was also applied to the control group (N = 31), with the difference that, in this case, both series of ten words were presented to the subjects only in the waking state. Thus, the task of reproducing and recognizing the two series of words was observed under conditions where no hypnotic amnesia intervened.

In a total of 16 subjects, the following experimental variant was also applied:

Before inducing hypnosis, the experimenter pronounced twice each word in a list of ten words. Another experimenter unknown to the subjects then pronounced twice, during hypnosis, each word in another list of ten words. After terminating hypnosis, the procedure already described in the case of the first experimental variant was used.

The results are given in Tables 1, 2 and 3.

Table 1. *Experimental group (number of words as an average)*

N = 31

Words reproduced		Words recognized	
Series I Awake	Series II Hypnosis	Series I Awake	Series II Hypnosis
2.0	0.5	6.3	3.3

Table 2. *Control group (number of words as an average)*

N = 31

Words reproduced		Words recognized	
Series I Awake	Series II Awake	Series I Awake	Series II Awake
1.5	2.1	6.0	6.0

Table 3. *The epxerimental group in which the second series of words was presented by an experimenter unknown to the subjects*

N = 16

Words reproduced		Words recognized	
Series I Awake	Series II Hypnosis	Series I Awake	Series II Hypnosis
2.3	0.4	5.3	2.3

The comparison of means has been made by the *t* (Student) test, of the difference between the average figures. As the total numbers available represented small numerical values the following formula was used:

$$x^1 = \sqrt{x + \frac{1}{2}}$$

a) All the subjects of the experimental group affirmed that they had been given only one series of words, namely, before they were brought into the hypnotic state. In all these cases, amnesia of information was observed from then on.

b) On the occasion of recognition, and sometimes also on the occasion of reproduction, the subjects of the experimental group also reactualized some of the words which had been communicated to them during hypnosis. (Of the 31 subjects, only 3 did not recognize a single word of the second series of words communicated during hypnosis.) Since the subjects were recalling information without knowing its origin, we can say that, in this case, we are dealing with an example of source amnesia.

c) The number of words reproduced and recognized was, of course, smaller for the second series (given under hypnosis) than for the first (given before the induction of hypnosis) ($p < 0.01$). In the control group there is little difference between the mean of words reproduced and recognized from either series ($p > 0.05$). A comparison of these data shows that source amnesia is accompanied by "partial amnesia of information" since the subjects in the experimental group reactualized on average fewer words from the second series than from the first.

d) The application of the variant in which the second series of words was presented by a person other than the one inducing hypnosis demonstrated that, under these experimental conditions too, the subjects recognized and sometimes even reproduced words transmitted in the course of hypnosis. A comparison of Tables I and II shows that the proportion of words recognized and reproduced is almost equal for the two experimental groups ($p > 0.05$).

e) The fact that the performance of reproducing and recognizing the words of the first series, presented during the waking state, is almost equal for experimental and control groups ($p > 0.05$) shows that the hypnotic state did not affect the memorizing of the words heard by the subjects before the induction of hypnosis.

II. Picture Test

The subjects ($N = 21$) were shown under hypnosis ten pictures, each representing an easily identifiable object. In order to assure ourselves that the subjects adequately perceived these pictures, we asked them to name each of the objects. After annulment of the hypnotic state, the following procedures were used:

a) The ten original pictures were first presented mixed with another 30 which had not been shown before, the subject being asked to state whether he had seen them previously, and upon what occasion.

b) The subjects who denied having seen these pictures were requested to select ten pictures from the total of 40 and were told that these would be handed over for copying to a schoolboy of 9—10 years.

c) The subjects who, even on this occasion, did not choose mainly the pictures seen by them when under hypnosis were requested to make a final selection, the inducement given by the experimenter being the following:

"Certainly you are more familiar with some of the pictures which are before you. Indicate those which you feel sure you have already seen at some time."

Each of these procedures represented independent tasks, and the subjects were not given any appreciation regarding the selection made. These successive stages were aimed at "forcing" the reactualization of the information transmitted intra-hypnotically, from spontaneous recognition to that indirectly guided. The results were the following:

Table 4

Procedure used	Number of subjects	Mean percentage of pictures sel. from the series of 10 pict. pres. during hypnosis	Mean percentage of pictures sel. from the series of 30 pict. added after hypnosis
a	8 (38%)	90%	—
b	6 (29%)	90%	8%
c	7 (33%)	80%	3%

The results were assessed by the method of variant analysis (the block method).

a) The groups differed from one another ($p < 0.01$)
b) The groups differed from one another ($p < 0.01$)
c) The groups differed from one another ($p < 0.01$)

The individuals did not differ from one another.

38%, examining the series of 40 (10 + 30) pictures which were shown to them immediately after the annulment of hypnosis, spontaneously selected only those pictures which had been shown to them during hypnosis. They claimed to have seen them in a dream (sometimes they also made up a story), or that they had seen them upon some other occasion.

29% selected mainly the pictures which they had seen under hypnosis, after the "forcing" procedure "b" had been applied.

33%, representing the remainder of the subjects, selected pictures amongst which those seen during hypnosis were clearly predominant, after the application of procedure "c".

Of the results obtained, two are of special interest:

1) The process of recall, which occurred in each of the three successive stages, was accompanied by amnesia of the source of information.

2) The source amnesia manifested itself in several ways, which represent different aspects of the process of amnesia: From recalling certain information without remembering the circumstances under which it had been obtained (the subject was referring to pictures seen at some time), to a recollection which the subject did not recognize as such; in the latter case, although he selected predominantly pictures previously perceived during hypnosis, the subject seemed convinced that he was choosing pictures which he was seeing for the first time.

Discussion

1) From our experimental data, it is apparent that in all cases in which posthypnotic amnesia had initially been observed there could also subsequently be demonstrated amnesia of the source of information. This amnesia has a relative character, a fact that is also borne out by the ability to reproduce and recognize posthypnotically certain information, without remembering its origin. It would appear that the subject who refuses to admit having received information during hypnosis will very shortly afterwards fail to show the capacity to recall this information. Such an opinion is proved to be justified when, for instance, the behaviour of the subjects is examined when they are required to simulate the state of hypnosis. As has recently been shown by WILLIAMSEN *et al.* (1965), these subjects tend to exaggerate their role, and prove to be much more affected by amnesia than those who have not been asked to simulate hypnosis. The hypnotized subject does not prove to be "consequently" amnesic. He shows a peculiar behaviour pattern, expressed in various ways, including a tendency to contradict himself overtly. ORNE (1959), describing the behaviour of the hypnotized subject, finds in him, amongst other things, the presence of a so-called trance-logic, i.e. a manner of answering without any apparent attempt to satisfy a need for logical consistency. While denying the perception of any information transmitted intra-hypnotically, our subjects none the less unhesitatingly reactualized it as soon as the experimental procedures created favourable conditions for deblocking stored information. For this, a subjective logic suffices, a "rationalization" which is indirectly suggested by the procedure used or insinuated unwittingly into the subject's mind by the instruction applied.

Our data differ from those of EVANS and THORN (1966), who very rarely found both types of posthypnotic amnesia in the same subject. This may be explained in part by the fact that, in our study, we applied various procedures for "forcing" the reactualization, with a view to overcoming amnesia. The possibility cannot, however, be excluded that this lack of agreement may have been influenced by differences in the characteristics of the respective experimental groups.

2) The fact that both the amnesia of information and that of the source of information can be observed in the same person is no reason for ignoring the difference between these two manifestations of amnesia. Our experimental material does not, however, provide any data concerning the nature of these differences. From a strictly phenomenological point of view, the difference is obvious. It is all the more apparent inasmuch as, in the usual process of forgetting, we are constantly confronted with analogous phenomena; either we permanently forget certain kinds of information or else, while retaining the capacity to evoke it, we are not aware of its origin.

(There are certainly numerous factors which facilitate the appearance of one or other of these manifestations, such as the specific nature of the information, the reasons for which it has been transmitted, the motivation of its acquisition, etc.)

It is characteristic of the two forms of posthypnotic amnesia, as emphasized by our experimental data, that through the indirect procedures utilized, one type is transformed into the other; that is to say, the information amnesia is transformed into a source amnesia. The deblocking of the information takes place, but not the annulment of the amnesia about the context in which this was transmitted. (By utilizing other procedures, the annulment of the source amnesia could also be effected.)

It is even possible that the appearance of posthypnotic source amnesia may be influenced by the specific nature of the suggestive effects. If the subject's capacity for transposition is relatively strong, there is a favourable condition for the information, once it has been transmitted, to impose itself as an independent factor, the suggested events being experienced as quasi-genuine events. The emphasis is placed on the effect by the suggestive stimulus, and not on the fact that this has acted as an excitant. It is not the act of transmitting, of communicating the information, which is significant, but the information itself, by reason of its content and affective effect. Our research on age regression has shown that the subjects who (after the annulment of hypnosis) clearly asserted that they did not remember anything of what had happened while they were under hypnosis, were in fact indirectly recalling these events. When asked to describe their dreams, they integrated oneirically without any hesitation some of the facts they had experienced during hypnosis. They described different scenes from childhood, school, etc., which corresponded fully to the chronological age and to the situations suggested.

The examination of posthypnotic amnesia of information, and that of source of information, reveals a fairly close relationship between the two. The amnesia of information also represents implicitly a way of confirming source amnesia. The amnesia of information is elicited, as a rule, by direct questioning of the subject on the events which took place during hypnosis. The usual answers obtained on such occasions are: "You didn't tell me anything", "I don't remember anything happening". Depending upon the intensity of hypnosis, and implicitly associated with the subjects' preconceived ideas about these phenomena, on returning to the normal waking state they sometimes have the impression that they are awaking from sleep (subjective sleep). To this must be added that they frequently expect, even in the absence of earlier special instruction in this respect, to experience amnesia with regard to events occurring under hypnosis. Consequently, they reject the idea that any such communication could have taken place and, as had already been observed long ago, they seem, in general, little preoccupied about what takes place during hypnosis. Under these conditions, the attempt of the experimenter to provoke recall of the information transmitted during hypnosis is met by a strong resistance which is further reinforced by every new attempt on his part. The subject, unaware consciously of the source, is also unaware of the existence of the information. But, inasmuch as the latter has been established as such, it can be brought to the surface by procedures which avoid the source. The manifest ignorance of the context in which the information was acquired, thus becomes the condition for its reactualization.

From our experimental data, it appears that information amnesia is sometimes manifested by a lesser capacity for reactualization than is usually encountered. The number of words reproduced and recognized from the series of ten words communicated to the subjects under hypnosis was found to be much smaller than that of the words reproduced and recognized from the series shown before the induction of hypnosis. In this case, one might perhaps speak of a partial information amnesia. The fact that these observations are supported by experience relating to source amnesia also illustrates, to a certain extent, the connection between the two forms of posthypnotic amnesia.

It is difficult to provide a precise explanation of the actual facts observed, i.e. that the number of words recalled from the second series is clearly smaller than that of the words recalled from the first series. It might nevertheless be surmised that a certain dissociation takes place between the two series of words, the subject unconsciously avoiding the words perceived under hypnosis. By affirming the existence of only one series, namely, that communicated before hypnotization, it is possible, at the moment of reproducing and recalling, for the subject to be oriented selectively towards the recall of that particular series. To check the capacity to discriminate beween the two series of words, we arranged another control group (matched to the other two). The subjects comprising this group, to whom the second series of words had been communicated in the waking state, were given the task of reproducing and then recognizing the words which had been communicated to them in the first series. One-third of the subjects stated that they could not carry out this rask. The remainder asked to be given the word first, and then to be allowed to ascribe it to the first or second series, respectively. The mean number of words attributed by these subjects to the first series was much smaller ($p < 0.01$) than the mean number of words recognized from both series by the subjects of the experimental group; at the same time, the number of words attributed by them to the first series, but which really belonged to the second series, was much smaller than the number of words reactualized from the second series by the subjects of the experimental group. If the subjects of the experimental group, i.e. those to whom the second series of words had been presented during hypnosis, had succeeded in the first place in concentrating upon the words of the first series, the proportion of words reproduced and recognized by them would have shown close approximation to, and only slight divergence from, the number of words attributed by the subjects of the control group to the first series of words.

The most probable hypothesis would seem to be that the reactualization on a smaller scale of the words communicated during hypnosis, as compared to those transmitted before its induction, is due to the differing conditions of fixation. In the waking state preceding hypnosis, the subject would be more attentive in receiving the words communicated. In the first place, there was the novelty of the task. In hypnosis the subject, being in a state of relaxation, did not manifest any interest in the stimulus presented, because this made no special demands on him—all that was required of him was to repeat a few words. The level of attention was also lower, because the stimulus did not present any new feature. Thus the difference in the manifestations of posthypnotic source amnesia draws attention, not so much to the phenomenon of amnesia, but rather to certain peculiarities specific to the hypnotic phenomenon itself.

3) As to the question whether there is only one or whether there are several forms of manifestation of source amnesia, our experimental data reveal the existence of several forms. One of these, mentioned above, is the source amnesia implicit in information amnesia. (The subject does not remember the information, because he rejects the idea that it is possible to communicate with him during hypnosis.) Another form is that where, after hypnosis, the subject evokes the information that was intra-hypnotically transmitted but attributes its source to extra-hypnotic experiences, such as dreams, information acquired long ago, etc. The absence of information amnesia, simultaneously with the maintenance of source amnesia, can be demonstrated, however, not only by an erroneous reproduction of the source, but also by the inability to indicate any source.

Finally, a third form occurs when a subject who has reactualized the information perceived during hypnosis behaves as if this information was a spontaneous creation of his own mind and, consequently, different from information acquired from any other source. This form, which is analogous to unconscious plagiarism, situates the source in the person of the subject himself. This is even more apparent in the results obtained from the picture test.

4) The stage of hypnotic amnesia cannot be detached from the problems of the wider field of the psychology of consciousness and its various levels. Some experimental studies have recently been carried out, relating more particularly to the connection between amnesia and the problems of levels of consciousness (Granone, 1965; Orne, 1966).

The study of amnesia and of the processes of memory in general, in the intra- and posthypnotic states, can help to elucidate some aspects of the relations between the conscious and the unconscious states, in precisely the same way as the memorizing or storing of information takes place in one particular functional state of consciousness, and its recognition or reproduction in another specific functional state.

Posthypnotic reactualization of information acquired in the hypnotic state, while the amnesia relating to the source of this information is maintained, suggests the possibility of unconscious recognition and reproduction. When the subject reproduces posthypnotically the information acquired during hypnosis in the belief that it is the product of his own mind, it seems to us natural to speak of unconscious reproduction. The problem of unconscious recognition is more difficult. Recognition appears to be a phenomenon whose very essence is the conscious confrontation of the stimuli perceived in the present with the images perceived previously. But it may be surmised that, up to a certain level, this confrontation can also take place unconsciously. This is the case in the posthypnotic state when, in the presence of a larger number of visual stimuli, the subjects select predominantly those which have acted in the hypnotic state, without being consciously aware that they have previously perceived these stimuli.

Obviously, one cannot exclude the possibility that this process of unconscious recognition may also be stimulated by the unconscious desire of the subject to react in accordance with the experimenter's wishes or, in other words, in accordance with what he imagines the experimenter might require of him. It may be that different ways of interpreting the unuttered demands of the experimenter could provide an explanation of the individual differences encountered in the application of the picture test. Despite this possible motive, the fact remains, however, that recognition is not operating on the conscious level.

Summary

The experimental procedures used in our research demonstrate the possibility of transforming the posthypnotic amnesia of information (recall amnesia) into posthypnotic source amnesia of information, as well as the existence of some complex modes of interaction between these amnesic forms.

Posthypnotic source amnesia is considered as a manifestation of unconscious reproduction and recognition.

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Experimental Psychology and Hypnosis*

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The relationship of hypnosis to academic psychology has through the years been similar to its relationship to psychiatry—sometimes popular and supported by those of prestige within their disciplines, sometimes in disrepute and carried on only by those on the fringes. This cyclical history is well known, and we suffer a little even today as we attempt to maintain interest in hypnosis within the scientific community, although many signs, such as the IBRO-sponsored conference in France in October 1967, indicate a more favourable contemporary attitude.

If we go back to 1890, when WILLIAM JAMES wrote his *Principles of Psychology*, and unashamedly included a chapter on hypnotism, we find that the phenomena of hypnosis were already well known: amnesia, suggestions of muscular performances, hallucinations, posthypnotic suggestions, analgesias, hyperaesthesias. The main trouble then, as now, was that a great deal of unproven folklore was included within the tradition, and reported by JAMES as simple truth.

Experimental psychology's contribution to the study of hypnosis has been primarily critical, that is, to separate by the use of more sophisticated controls what is true from what is false in the folklore that has grown up around hypnotic phenomena. The early experimenters—men such as BINET and FÉRÉ, DELBOEUF, and JAMES himself—were not sufficiently versed or skilled in the experimental method to detect subtle errors introduced in experiments which, in some formal sense, were intended to provide controls. Thus a magnet might be wrapped in cloth, on the supposition that the experienced hypnotic subject would not know that it was a magnet. Years later, HULL (1933), in re-reading BINET and FÉRÉ (1888), was shocked by their bad experimental techniques, for both were men of intelligence and training, yet overawed by the authority of CHARCOT, and convinced of the residual effectiveness of magnets.

This critical role of the laboratory is a useful one, but it would be too bad if that were all that a scientific laboratory approach could contribute.

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Truly important science is always an exercise of the imagination, and leads to invention and discovery in addition to sorting truth from falsehood. It would be a poor day for psychological science if all the truths of hypnosis had been discovered by the early amateurs, with the later workers only saving the more believable from the less believable. However, if this sorting were all that experimenting achieved, it would still be a genuine service; it may even be that this is about as far as we can go today, for the experimental study of hypnosis has in fact been quite limited to the present time, and much effort has had to be devoted to clearing away persistent false beliefs. The methods of the experimental laboratory in psychology are not themselves infallible, so that some of the effort is dissipated in settling disputes between contemporary experimenters. The problems of experimenter bias did not die with the nineteenth century, and a recent book by ROSENTHAL (1966) has shown how insidious this bias is in contemporary experiments having nothing to do with hypnosis: even demonstrating experimenter bias in that favourite concern of American experimental psychologists, the study of the white rat in the maze. Thus, if rats matched for ability have their cages marked "bright" and "dull", those marked "bright" tend to score better in the maze experiment.

The book which for its time best represented the more modern experimental approach to hypnosis is that of CLARK L. HULL (1933), reporting a series of experiments chiefly by his own students and collaborators. Professor HULL went on to a distinguished career in the development of a learning theory, and his abandonment of hypnosis after writing his book unfortunately may have cancelled much of the stimulating effect his book might otherwise have had upon experimentation by his students and by other experimental psychologists.

As we come somewhat nearer to the present time, we have the book by WEITZENHOFFER (1953) reviewing the experimentation that had been done in the two decades after HULL wrote. Some two-thirds of the 508 references cited appeared later than HULL's book. While the amount of work was substantial in quantity, much of it was not of high quality by modern experimental standards, and there were many internal contradictions to be resolved.

I wish now to refer more particularly to the work of the last 10 years or so, when, with more modern equipment and with better resources, an increasing number of investigators in various parts of the world have been using the experimental method to help us understand hypnosis. When I speak of the experimental method in psychology, I do not wish to limit myself to narrow methods historically called experimental, but rather to methods that are in the spirit of the laboratory, involving control of conditions and objectivity of recording, so that others can repeat the investigations for themselves. Sometimes the methods will be physiological, sometimes psychophysical, sometimes they will involve verbal interactions. Perhaps the sharpest contrast is with ordinary clinical observations, in that the subjects for an experiment come at the invitation of the experimenter, and do not come as patients for the help that they may receive. There are some areas of overlap here, however, and there is nothing to forbid experimentation with subjects who are at the same time patients, such, for example, as those suffering from painful burns, or the pains of terminal cancer. I consider the genius of experimental psychology to reside in its attitudes and rigour, not in its subject-matter, whether defined by the persons tested or the topics of inquiry. Having thus stated my general position, I wish to select a few topics representative of studies that have been made in more recent years.

Experimental Approaches to Hypnotic Phenomena

Let me proceed by indicating a few problems that we have been trying to clear up. This must of necessity be a limited sample of what has been done, which in turn is a limited sample of what might be done. It is of some interest to recall the 40 projects on waking suggestion that HULL proposed in 1929 (HULL 1929), and the 102 projects outlined by him the following year (HULL, 1930), almost 40 years ago. They show how ingenious HULL was in translating problems into experimentable form, but they show also how slow we have been in following up his suggestions. I shall not attempt a review of his topics here. Instead, I wish to use five topics as illustrative of efforts to make objective various scientific approaches to problems that have long been in the folklore of hypnosis. These five are (1) the nature of susceptibility, (2) the effects of induction upon suggestibility and depth of trance, (3) amnesia, (4) analgesia, and (5) hallucinations.

1. The nature of susceptibility. It has always been recognized that some people are more readily hypnotized than others, but folklore goes on to make two additional assertions: (1) that a more highly skilled hypnotist will be able to hypnotize a larger fraction of the subjects than a less highly skilled one, and (2) that a subject can be trained to become a more susceptible one. These two statements are of a kind which can be true in some respects and not true in other respects; the truths they represent can be clarified only by quantitative investigation.

A number of careful studies have been made of the differences in susceptibility to standard hypnotic procedures, and scales have been developed to assess these differences. The work from our laboratory and a number of others is reported in some detail in my book (HILGARD, 1965b), and need not be gone into here. It is demonstrated that hypnotic susceptibility under ordinary laboratory conditions is a fairly consistent characteristic of the individual, as shown by reinductions on other occasions with different hypnotists; hypnotizability, considered as a personality trait, is heavily loaded with a common factor, although there are secondary factors so that, of two moderately susceptible subjects, one may manifest some phenomena while another will manifest other phenomena.

The two further statements about hypnotist skill and change of susceptibility with practice require supplementary tests. Our evidence, such as it is, makes hypnotist skill relatively unimportant. Success appears to be about as great with beginners as with experienced hypnotists, and susceptible subjects respond very well to inductions delivered by an unfamiliar voice on electromagnetic tape, with what is said having nothing to do with capitalizing on their responses. Such results must be stated tentatively, however, because in the laboratory there are certain uniformities of expectation that would not be met, for example, in clinical medical practice, where technique and transference effects may be more important. This remains something to be demonstrated, however, and it is strange how limited the evidence is. Some hypnotists, such as MEARES (1967), who claim to hypnotize everyone, have very minimal criteria of what constitutes hypnosis, and it appears increasingly clear that the degree of hypnosis necessary for its use in psychotherapy is very slight indeed. Such evidence as we have is that hypnotist skill is not a very important variable, although further study is needed, possibly with dramatically contrasting techniques. It might turn out that skill in choosing the technique appropriate to the subject studied would be important, particularly in clinical practice (DORCUS, 1963).

The matter of training the subject to become more hypnotizable is another aspect of hypnotist skill. The first thing that must be said is that, on repeated sessions, a generally susceptible subject goes into trance more quickly, as is well known, even at a prearranged signal. This fact may make the hypnotist appear to be very skilled, but the further question needs to be asked, whether the subject has indeed become capable of a deeper experience of hypnosis. Several careful experiments have led to the conclusion that it is very difficult indeed to produce deeper involvement in hypnosis unless relatively satisfactory involvement is there practically from the start. This is the essence of the findings from our laboratory by ÅS, HILGARD, and WEITZENHOFFER (1963), and from Dr. ORNE's laboratory as reported by SHOR, ORNE, and O'CONNELL (1966). In a later extension of our earlier study, however, we found that the gains made with practice were a function of the initial performance, so that a moderately hypnotizable person can be made a much better one, while an insusceptible one is very hard to modify (COOPER *et al.*, 1967). It is quite possible that most of those who "train" subjects actually begin with fairly susceptible ones in the first place, thus supporting the folklore with respect to improving susceptibility with practice. More recently in our laboratory, SACHS and ANDERSON (1967) have had more success than we had earlier in improving susceptibility through careful training of individual responses until the subject learned to produce and experience hypnotic-type responses as a susceptible subject does. For example, by piling books on a hand placed on the table, the subject learned to experience what a "heavy" hand felt like when you tried to lift it. This "feeling" was then practised as the books were gradually removed; later, on the suggestion of heaviness, the hand "really" felt heavy as it does for a susceptible subject. Once the subject learned to have this kind of "involuntary" response to suggestion, it generalized to other kinds of experiences expected of a more susceptible subject. The experiment is not very convincing when described in this way, but there are indications, as from the excitement of the subject when he finds that he actually has a contracture, that the training did more than increase compliance. The experiments are continuing, but the issue remains unresolved, whether or not susceptibility can commonly be changed, and to what extent. We can certainly improve the response of some subjects by correcting their resistance to hypnosis, but the problem is more difficult and the answers less certain for cooperative subjects who cannot be hypnotized despite their desire for the experience. The answers to this question are obviously important, because if we can train *anyone* to become hypnotically susceptible, the advantages of hypnosis to the individual (as in the relief of pain) can become more generally available.

2. The effects of induction upon suggestibility and depth of trance. The practice of hypnosis makes the assumption that the hypnotist, through the conditions that he sets up, or his relationship to the subject, causes the subject to enter into the hypnotic state or "trance", and that this state has various characteristics, including increased responsiveness to suggestions. The usual assumption is that, with a more protracted induction, the trance can be deepened. GILL and BRENMAN (1959), for example, following up the suggestions of KUBIE and MARGOLIN (1944), have given an account of how the normal ego is disrupted during the progress of induction, and a new temporary reorganization takes place within the trance, which is in some respects a regressed state. Regardless of the several ways in which the hypnotic state can be characterized (*e.g.*, CHERTOK, 1967), most of us would agree that it exists, so that it comes as something of a surprise to find that the effectiveness of induction is challenged, and that the experiments directed to the problem are not wholly unambiguous in their findings.

Let me first of all distinguish between this problem and the one previously discussed. People might differ in their responsiveness to suggestions (as indeed they do in response to waking suggestions) regardless of what we find out about induction. The problem of *depth* of hypnosis following induction is separable from the problem of *susceptibility*. A person who is highly susceptible can be hypnotized to any degree intermediate between the waking state and the asymptote which he is capable of reaching. So we have a different problem in discussing depth as a consequence of induction, and susceptibility as a limit of hypnotizability under standard conditions.

In so far as most of us have witnessed hypnotic inductions, and the changes taking place, it seems rather ridiculous to deny the reality of induction. However, it might be that the behaviour in induction is itself a response to suggestions given in the normal state (eye closure, relaxation, appearing to be in a sleep-like condition), and it does not *necessarily* follow that this state is an essential preliminary to other phenomena of hypnosis, such as amnesia, or responsiveness to posthypnotic commands. The one who has taken this position most strongly in recent years is T. X. BARBER, a prodigious experimenter who has adopted as his central task the disproof of the effectiveness of induction (*e. g.*, BARBER, 1965). He does not deny individual differences in suggestibility, or many of the behavioural phenomena associated with hypnosis, although his scepticism is very great regarding any effect beyond compliance or role-playing. I believe that his assertions are in many cases beside the point, and TART and I performed a series of experiments in which we attempted to show in what ways his results were misleading (HILGARD and TART, 1966; TART and HILGARD, 1966).

These experiments were concerned with responsiveness to waking and imagination instructions, compared to responsiveness to the same items following hypnotic induction. In agreement with the work of BARBER and of others, we found substantial correlations (of the order of $r = 0.70$—0.80) between responsiveness in the waking-imagination conditions and in the hypnotic conditions, so that, overall, the effects of induction are not as great as might be expected, even on such items as posthypnotic amnesia. But we did note some differences that show the care required in designing experiments of this sort. There are, first of all, a few highly susceptible subjects who respond very well *without* induction as well as *with* induction. In addition, there are many relatively insusceptible subjects who do not respond any more with induction than without induction; they give statistical support to BARBER's contention, without really being pertinent, because they are simply not hypnotizable, and one does not expect induction to accomplish anything. Because of this fairly large proportion of subjects (at the top and the bottom of the scale) who will be consistent from waking conditions to hypnotic conditions, it is expected that with small groups of subjects unselected for hypnotic susceptibility it will be difficult to show significant effects of induction, the result that BARBER and his collaborators commonly celebrate. However, with what we considered a more appropriate experimental design, sensitive to smaller differences, we were able to detect a *third* and *inbetween* group of subjects who showed rather pronounced changes between their waking responsiveness and their responsiveness within hypnosis. Their changes were sufficient, indeed, to yield statistically significant comparisons for the group means, in favour of an increase of suggestibility with hypnotic induction. It must be said, in fairness to BARBER and his co-workers, that the average changes found are indeed not striking, and probably less than most of us would have expected on the basis of our intuitions about hypnosis.

The problem of the actual effects of trance-deepening procedures has not yet been solved. This requires some sort of probe that can be given, to determine objectively just how deeply hypnotized the subject is, and then to measure his responsiveness at that depth. The experiments of ROJNOV (1967) are pertinent, but it is not clear that they could be used for the present purpose. We have been totally unsuccessful in reproducing the kind of cyclical phenomena that KATKOV has scaled, as reported by PLATONOV (1959). I say this regretfully, because something of the sort would be very useful. We have used verbal reports in the form of numbers from 1 to 10 with some degree of satisfaction in getting reports of hypnotic depth, but have not yet found entirely satisfactory correlates. We have reported some preliminary results with hypnotic analgesia with one subject, in which the amount of analgesia to placing the hand in ice water was indeed correlated with the depth of hypnosis (HILGARD, *et al.*, 1967), but we have not yet determined how to correct for the subject's expectation that with lighter hypnosis there should be lesser effects. This is a fascinating experimental problem, to which further attention will be directed, especially if we are able to find stable physiological correlates of hypnotic depth.

3. Amnesia for the trance. One of the marks of the highly hypnotizable subject, often termed a somnambulist, is that he is spontaneously and completely amnesic to events within the trance. This is a folklore statement also, and it, too, has been challenged by experiment. From what we now know about experimenter bias, it is quite possible that spontaneous amnesia arises from the subject's expectations that this is what is called for, possibly by the manner in which the interrogation is done, possibly from the inference that hypnosis is like sleep, and you do not remember what happens when you are asleep. In any case, our experiments along these lines have shown very little spontaneous amnesia, although quite satisfactory amnesia is found when it is suggested (HILGARD and COOPER, 1965). It has been known all along that even a subject usually amnesic for the trance can remember trance experiences when told that he can; it is not implausible, therefore, that the amnesia itself is a result of some sort of special implied suggestion. The contribution of experimental psychology to this problem comes through the careful specification of the conditions under which amnesia is suggested, and the careful measurement of the degree of memory loss involved.

In the experiments of EVANS and THORN (1966) we have an illustration of the positive contribution of the laboratory, beyond its merely critical function, in that these investigators helped to separate two kinds of amnesia: *content* amnesia and *source* amnesia. They found that some subjects would remember something learned under hypnosis, but would forget that they had learned it there: this is *source* amnesia. For example, if taught the population of some city of the world whose population was previously unknown to him, a subject might state the correct population figure after hypnosis, but either be surprised that he knew it, or rationalize his knowing it on some unlikely basis. In complete amnesia the information, too, would be gone. COOPER (1966), in our laboratory, was able to demonstrate such source amnesia, both spontaneous and suggested. GHEORGHIU (1967) has also reported work along these lines, adding some other types of amnesia to the familiar ones.

4. Analgesia. The reality of pain reduction under hypnosis appears amply attested by the amount of major surgery that has been done over the past century with the help of hypnosis, by its use in childbirth, in dentistry, and in the relief of the pain of migraine headaches, burns, or cancer. Yet the sceptical experimenter has found ways of denying this also (SUTCLIFFE, 1961; BARBER, 1963). One wonders whether denying facts so well established is not a caricature of the experimenter's critical attitudes; yet it may be argued that it is his business to question even things that seem obvious, and thus to force the believers to produce better evidence.

The kinds of scepticism expressed are twofold. First, there is sometimes an assumption that the subjects are merely "heroic" and do not admit that they have pain. This is an assumption that is entirely false in our experience, and in the experience of many others. I believe that a subject can be absolutely without any experience of pain, even though exposed repeatedly and for a long period of time to noxious stimuli that would normally be experienced *by him* as painful. The second assumption is that pain can be counted only if it is registered physiologically, and that absence of pain has meaning only when these physiological signs of pain are absent. This sounds simple enough, but it is very troublesome because there are no simple indicators of physiological pain that are not also indicators of other, psychological processes, and a physiological indication that a subject has received stimulation does not mean that he has experienced pain. We now have evidence from cold-pressor responses that those simulating absence of pain yield different physiological measurements (especially changes in finger blood pressure) from those relieved from pain by hypnotic analgesia, and we have evidence from another form of ischaemic pain, produced by a tourniquet and exercise, that the rise in blood pressure associated with pain in the waking state is absent in hypnotic analgesia. We therefore have physiological evidence for hypnotic analgesia, although I do not believe that such evidence is needed to establish the reality of hypnotic analgesia. The evidence is desirable to show what is happening within analgesia, not to prove its reality. (For preliminary reports, cf. HILGARD, 1967; HILGARD *et al.*, 1967.)

5. Hallucinations. What I have been calling the folklore of hypnosis says that a well-established hallucination will be of perceptual intensity, and ought to be subject to the same parameters that control perception. Thus if a hallucinated colour is real, then a coloured after-image ought to follow the disappearance of a hallucinated colour. Or, if the subject sees the swinging of a hallucinated pendulum, his eyes ought to make the same pursuit movements that would be made if an actual pendulum were swinging before him. This is a problem well designed for experimental study with modern instrumentation for the study of eye movements. A study has been reported briefly by DECKERT (1964), indicating that *with eyes open* the hallucinating hypnotic subject makes numerous saccadic movements in following the hallucinated pendulum, similar to the movements made if he imagines such movement in the waking state. With eyes closed, however, the pursuit appears to be smoother, but the subject does as well in imagination as in hypnosis. The results, while somewhat confusing, do not lend support to the position that *in a physiological sense* the subject reacts as he would to an external source of stimulation. I do not see why be should, in view of the many reflex levels that are dependent upon sensory input; still it is important to know that he does not react in that way. This does not for me deny the reality of his hallucinations; it means only that hallucinations do not have the same sensory feedback as perceptions depending upon external sources of stimulation.

ORNE (1959) has made skilled use of hallucinations as means of differentiating between truly hypnotized persons and those simulating hypnosis, in that the truly hypnotized person adopts a kind of "trance logic" in which he is not troubled by persons being doubled, etc., while the simulator does not accept such inconsistencies. HORVAI and HOSKOVEC (1967) have reported additional experiments on these topics, with results indicating responses present to aspects of stimulation denied in negative hallucinations.

NARUSE in Japan has done some very interesting work in "conditioning" hallucinations (NARUSE and OBANAI, 1953; NARUSE, 1962) following some earlier work of LEUBA (1940). When an incidental conditioned stimulus (e.g., a buzzer) occurs along with a visual stimulus under hypnosis, after the subject comes out of hypnosis with amnesia for the experience, the posthypnotic sounding of the buzzer tends to produce the visual hallucination as a sort of "conditioned response". Some work in our laboratory, introduced there by NARUSE, shows these responses to be quite genuine, and subject to experimental manipulation. For example, if a background divided into a white half and a black half is produced through conditioned hallucination, gray circles on these backgrounds will show the expected contrast effects with naive hypnotizable subjects – (unpublished study by KENNETH R. GRAHAM).

I have given these brief illustrations of several topics in order to show the kinds of things that can be done in the laboratory to establish the conditions for clarifying what is true in the hypnotic tradition, both in respect to hypnotic induction and trance characteristics, and in relation to the reported manifestations of hypnosis. I have left out a great many topics, some of which I have reviewed elsewhere (HILGARD, 1965a).

Experimental work continues on many topics: anxiety and stress, their generation and reduction under hypnosis; the personality correlates of susceptibility; age regression and hypermnesia; feats of strength, precision, or endurance; sensory acuity, habit acquisition, and memory; posthypnotic suggestions of various kinds and durations; hypnotic dreams in relation to night dreams; sleep learning; attention and its physiological correlates; the reduction of evoked cortical potentials in hypnotically induced blindness, deafness, and analgesia.

These topics suggest the rich harvest that awaits investigators in this field, now that many new approaches to the problems of hypnosis are possible. The list also leads us to the next part of this paper, because if we had the answers to all of the questions raised, we would know more about psychology generally as well as more about hypnosis.

The Contribution of Hypnosis to Experimental Psychology

If we turn our interest in the opposite direction, and think of advancing the scientific maturity of experimental psychology, rather than of advancing our knowledge of hypnosis, we find that hypnosis can make a substantial contribution. The point is simply this: If a legitimate psychological question can be answered better by using hypnosis than by using some other approach, then hypnosis should be used. This is not hypnosis for the sake of hypnosis, but hypnosis for the sake of advancing psychological knowledge.

A few selected topics will help to illustrate what I mean. I have chosen to talk about (1) the nature of dreams, (2) the influence of states of arousal upon cognitive processes, (3) information processing and memory storage, and (4) the specificity doctrine of psychosomatic effects.

1. The nature of dreams. Psychologists have in the past not been very successful in studying dream contents, despite the interest aroused by FREUD. Now, however, the discovery of the characteristic eye movement pattern within sleep at EEG-Stage 1, commonly called the REM state, has caused a great flurry of scientific interest. The work of GASTAUT and JOUVET in France, and of DEMENT at our own university, is well known, and many workers around the world are participating.

At the various meetings of the Association for the Psychophysiological Study of Sleep there are almost always some papers in which hypnosis has been used in connection with these studies. Hypnosis can be used to produce dreams, to specify the topics to be dreamed about, to cause waking at the time of dreaming, to facilitate recall. These attempts are not always successful, but there are sufficient successes to justify the use of hypnosis.

Perhaps the most interesting single development in this respect is posthypnotic stimulation of sleep-talking while dreaming (ARKIN, HASTEY, and REISER, 1966). Through posthypnotic suggestions planted in the waking state it is possible to instruct the subject to talk about his dream while it is occurring, and then to sleep comfortably and silently when the dream is over. Thus, for the first time we have an account of the dream while it is going on, and do not have to rely upon reconstructions after waking. The remembered dream in the morning may be a fragmentary version of the full dream reported while it was happening. The method is not quite as easy as it sounds. It is necessary to find subjects who are both highly hypnotizable and have a tendency to talk in their sleep, and there is some controversy about what the simultaneous talking does to the dream state. Yet here we have a dramatic illustration of the use of hypnosis to investigate some problem beyond the problems of hypnosis itself.

2. The influence of states of arousal on cognitive processes. Many investigators have shown that it is possible to produce states of anxiety or arousal through hypnosis. At the University of Michigan, BLUM and his associates have conducted a series of very ingenious experiments making use of hypnosis to control the state of arousal, making their tests with several kinds of cognitive tasks (BLUM, 1963). The general procedure is to use very few subjects who have been trained, under hypnosis, to maintain particular states of anxiety or arousal at the appearance of a signal associated with each state. For example, states of minimal arousal may be generated by saying "minus AA" for minimal arousal, then —A, O, +A, and +AA, for five degrees of arousal, "zero" being normal arousal. After practice, in which the state is monitored by physiological measurements, the subject retains a posthypnotic suggestion to respond with this state whenever the letters are spoken, but to be amnesic for the instruction and to be unaware of the signal. Then other tasks can be presented in the waking state, with the assurance that the tasks will be tackled while the subject is in the appropriate state of arousal.

I shall give an illustration from a recent paper (BLUM, GEIWITZ, and HAUENSTEIN, 1967). In a particular method developed in BLUM's laboratory, the subject hears a series of six consonants, perhaps K-X-T-G-W-M, presented one at a time, at one second intervals, paced by a metronome. The subject merely listens, with no attempt to memorize the consonants. After the series is completed, the subject, not thinking of the consonants, turns to some other activity, such as counting aloud the beats of the metronome. At some predetermined time the subject is asked to recite any six consonants that come to mind, one at each beat of the metronome. Stress is laid upon the fact that the experimenter's concern is solely with the consonants that occur to the subject, whether or not they bear any relation to the previously presented ones. A score has been derived for measuring the degree to which the recited set of consonants corresponds to the presented one. For the purposes of the variation of the experiment that I am now describing, the subject hears the consonants, then begins counting to 20, after which he reports six consonants. But within the count, a pause of five seconds is introduced for the experimenter to announce the posthypnotic alertness signal, and at the end of the pause the experimenter tells the subject to continue counting, so that there will always be 20 seconds between the initial consonant presentation and the recall. Under these circumstances the consonant score turns out to form a perfect progression from the lowest score under the minimum arousal to the highest score with maximum arousal, even though these alertness conditions were introduced in random order *after* the exposure to the consonants had taken place. The authors suggest that states of alert arousal serve in some manner to "amplify" the *reverberations* that result from hearing the consonants and effect the eventual recall; ordinary hypnosis alone appears to be a state of very low arousal.

3. Information processing and memory storage. The experiments by BLUM and his associates bear upon information processing, but there are other ways in which this can be studied with the aid of hypnosis. There are many kinds of stimulus input to the organism which are doubtless *registered*, but remain *unperceived* (HILGARD, 1962). One proposal is that these unperceived sources of stimulation have persisting effects which are not detected until this unorganized material can attach itself to some kind of structure that enters consciousness, possibly in fantasy or in a dream. Experiments seeking to detect such after-effects of unreported stimulation occur in the literature of subliminal perception. Hypnosis can enter into experiments of this kind as a means of attempted retrieval of such unperceived (or unremembered) consequences of stimulation. We are undertaking some experiments of this kind. As an illustration of one context in which this use of hypnosis is feasible, I wish to mention an experiment concerned with the recovery through hypnosis of memories blocked by barbiturate sedation (OSBORN *et al.*, 1967). A subject who is sedated with thiopental (pentathol sodium) at a level that makes him drowsy but still awake, is able to carry on a conversation, to learn lists of easy material, to describe pictures shown to him, but after going to sleep and awakening may prove to be completely amnesic for the conversation, and unable to reproduce the list learned or to recognize the pictures seen. We were interested in finding out if the problem was one of memory storage or of retrieval, and it seemed appropriate to use hypnosis to this end, after a subject had shown himself to be amnesic. The reported results were not dramatic, but the amnesic subjects commonly recovered some picture recognition via hypnosis, although they seldom recovered the verbally memorized materials. The results indicated that more was in storage than was evidenced in waking recall, but the hypnotic recovery was far less than reports such as those of CHEEK (1966) and LEVINSON (1967) had led us to expect.

4. Specificity doctrine of psychosomatic effects. As my final illustration, I wish to cite an example of an experimental study that bears upon some psychodynamic conjectures that came out of the psychiatric clinic. The late Dr. FRANZ ALEXANDER, long at the Institute of Psychoanalysis at Chicago, some years ago developed a theory that people are vulnerable under strain to certain kinds of psychosomatic symptoms, and that particular personality profiles are associated with asthma, peptic ulcer, urticaria, and so on. A book by some of his associates is about to appear, with a good deal of evidence supporting this position, which is sometimes described as a "specificity doctrine" because it proposes that symptoms are personality-specific. One aspect of a related theory of this kind was tested under hypnosis by GRAHAM and his associates at the University of Wisconsin (GRAHAM, KABLER, and GRAHAM, 1962). The investigators had earlier found in their clinical studies that certain attitudes were associated with urticaria (hives) and with hypertension. These became the basis for an experiment on induced attitudes. The differentiating attitudes to be experimentally manipulated were appropriate to hives ("you feel that you are being unjustly treated and there is nothing you can do about it") and to hypertension ("you have to be on guard against being attacked and hurt"). It was conjectured that, when these attitudes were inculcated under hypnosis, skin temperature would rise under the "hives" suggestion, and diastolic blood pressure would rise under the "hypertension" suggestion. The results, in general, confirmed these conjectures.

Perhaps these brief examples suffice to indicate what I mean when I say that hypnosis may contribute to our substantive knowledge about psychology when our interest is not in the hypnosis itself, but in the role that hypnosis can serve as an aid in experimentation.

Conclusion

Through my illustrations of laboratory approaches to the study of hypnotic phenomena of susceptibility, of amnesia, of analgesia, and of hallucinations, I have tried to show that the experimental method can clear away the false beliefs that have persisted about hypnosis, can establish the sound features of it, and, hopefully, can add to our knowledge in significant and useful ways.

Through the subsequent illustrations of the use of hypnosis in studying dreams, arousal and cognition, information storage and retrieval, and psychosomatic relationships, I have tried to show further that hypnosis as a tool can advance our substantive knowledge in the fields of general experimental psychology, and of psychodynamics.

If we continue to build strongly along these lines, we may succeed in breaking the peculiar cycle of interest followed by lack of interest so familiar in the history of hypnosis, and succeed in maintaining and extending the present attention to hypnosis as one of the important areas for a scientific psychology to encompass.

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The Psychophysiology of Pain Reduction through Hypnosis*

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The Puzzling Nature of Pain

Despite the importance of pain, and the great scientific interest in it, it is surprising how uncertain our knowledge is, both of the psychological nature of pain and about its underlying physiology. Let me begin by a reminder of some of the puzzling problems.

1. First, surgeons have explored all sorts of operations in order to relieve pain, but no single "pain centre" emerges. I shall not go into details here; the review by MELZACK and WALD (1965) covers the problems very well.

2. Secondly, the effects of morphine, the most widely used pain-killer, turn out to be somewhat unpredictable. It appears, according to the work reported by BEECHER (1959), that about a third of surgical patients are relieved of their post-surgical pain by morphine beyond relief by a placebo, one-third are relieved by placebo as well as by morphine, and the final third are relieved neither by placebo nor by morphine (in doses believed to be safe to use).

3. Thirdly, there are many indications of social and psychological factors influencing felt pain. I shall mention three lines of evidence: (a) there are social class differences, so that members of the working class, when hospitalized, complain of pain more than members of the higher socio-economic classes (GONDA, 1962); (b) the amount of pain felt in laboratory experiments has been shown to be related to how much pay is offered the subjects: the higher the reward the greater the pain, even though the amount of reward is determined by lot (LEWIN, 1965); and (c) according to the block experiments of PETRIE (1967), experiments on kinaesthetic-tactual size estimation of wooden blocks can be used to select

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the augmenters, who magnify stimulus input, and the reducers, who diminish the effects of stimulus input, and these non-pain experiments predict that the augmenters will feel more pain and the reducers less pain when exposed to similar noxious stimuli.

4. There is, finally, the well-known relief of pain by way of hypnosis. I need not cite the history of the use of hypnosis in major surgery, in childbirth, in dentistry, and in the relief of the pain of burns. The clinical facts are so well established that it is rather surprising to find the confusion that exists in the laboratory study of pain relief through hypnosis.

This general situation with regard to pain caused us to undertake the experiments to be described.

Experiments Comparing Cold-Pressor Responses in Waking and Hypnosis

One of the favourite experiments in the study of pain consists in placing a hand (or foot) of the subject in ice water for a period of time, thus obtaining both pain reports from the subject, and concomitant physiological measurements, usually of heart responses from a non-stimulated arm. Pain mounts rapidly under these circumstances, reaching a maximum after about a minute—a pain that is nearly intolerable for many subjects, and then numbness sets in, reducing the pain, but also changing the quality of such pain as remains. If measurements are confined to the first minute the numbing effects can be ignored, although doubtless their preconditions are already being established. While the method is convenient, what is taking place at a microscopic level must be very complex indeed: a gradually more intense cooling of the arm or leg, changes in blood distribution with consequent local temperature changes, sensory adaptation, anaesthetization of some nerve fibres, and so on. The convenience conceals the complexity, and it may be a faulty assumption that whatever equilibration is achieved is comparable from one subject to another. The method was first brought to prominence by HINES and BROWN (1933), and a very useful early study is that of WOLF and HARDY (1941).

Our method was to take two sets of preliminary measurements: one in a condition described as *vigilance*, in which the subject was given the task of turning off one of two discriminable buzzers by pressing the appropriate one of two buttons, as the buzzers came on in random order; the other described as *relaxation*, a control condition for a relaxation similar to hypnosis, but with every effort made to avoid drifting into hypnosis. In the *hypnosis* conditions a relaxed hypnotic state substituted for the relaxation. These conditions determined baseline heart rates and breathing rates under an alert attentive condition and under a passive condition. Then the left hand and forearm were placed in circulating ice water. Verbal reports of pain were called for on a scale from "0" for no pain to "10" for a pain so great that the subject wished to remove his arm, though he could be persuaded to hold somewhat longer and to continue to count. The usual time in the ice water was 1 min. After removal and drying of the hand and forearm, the vigilant and relaxed conditions were repeated in order to study recovery.

I wish first to consider the adequacy of the verbal reports of pain as quantitative measures of felt pain.

Our data indicate that the data are orderly, and that the effect of time in ice water is equivalent to a geometrical increase in the pain stimulus as a function of time, as defined by the equation $p = at^k$ (HILGARD, 1967). When plotted as a log-log plot, the data fall along a straight line, corresponding to the familiar power function, said by STEVENS (1962) to be the most general psychophysical function. The value of k in the equation usually lies between 0.5 and 0.7.

The orderliness of the data, and the different times at which the critical pain level of "10" is reached, support the belief that these reports have indeed some objective meaning, but it would be well to have some external validation. We have conducted some side-experiments designed to test this validity; they are not reported here because the data have been incompletely analyzed, but I may mention the three lines of evidence we are using: (a) first, we have made use of a refrigeration device so that we can circulate water at various controlled temperatures. When we use water at 0°, 5°, and 10° Centigrade, we find very different pain-state reports, corresponding to the water temperatures on different days of experimentation, even though all the water feels very cold and painful to the subject; (b) secondly, we have attempted to compare the pain-state reports under drugs with water at a constant temperature. Our first experiment, with aspirin, was only mildly successful; we did indeed get some reduction in pain-state reports, but the reduction under placebo was equal to that with aspirin. Still, from the point of view of validating pain-state reports, the experiments were useful. (c) Using the block experiments of PETRIE (1967) to select augmenters and reducers in a non-pain experiment, we have attempted to see whether or not their pain-state reports are in the predicted direction. All the evidence taken together gives us considerable confidence in the use of the verbal pain-state reports as measures of felt pain, although some uncertainty still exists as to the most appropriate measure to use to correct for some individual idiosyncrasy in reporting.

Turning now to the reduction of pain through hypnosis, I wish to present three figures showing what happens to pain-state reports when a subject is hypnotized and given the suggestion that the immersed hand and arm will be insensitive to pain.

Two things are to be noted: first, the more highly susceptible the subject according to measures unrelated directly to pain relief, the more he is able to reduce pain through hypnotic suggestion (Fig. 1); and, secondly, there are enough individual differences among highly susceptible hypnotic subjects to ensure that some of the most susceptible are not able to relieve pain under the conditions of our experiment (Fig. 2). It should be said that the conditions are not optimal for the relief of pain, in that the experiences of hypnosis have been brief, no prolonged special training has been involved, and the induction of hypnosis within the experiment itself is very brief. However, the purpose is not to show how much analgesia can be obtained under the most favoured conditions, but rather to establish the analgesia under standard conditions which reveal the individual differences involved. With more susceptible subjects the pain reduction can be long continued, and is successful with posthypnotic suggestion as well as with direct suggestion (Fig. 3).

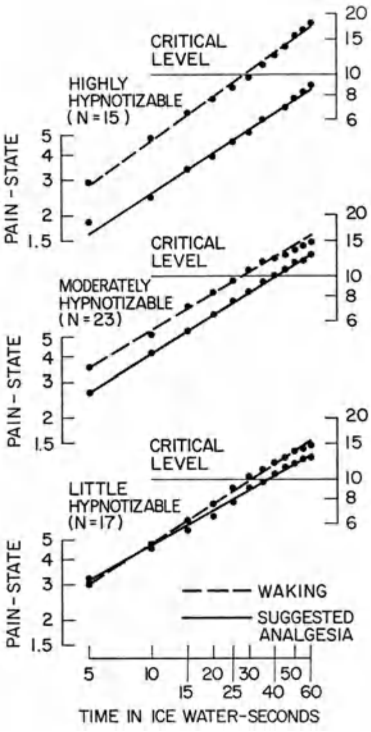

Fig. 1. Pain reduction under hypnosis as related to hypnotic susceptibility (HILGARD, 1967)

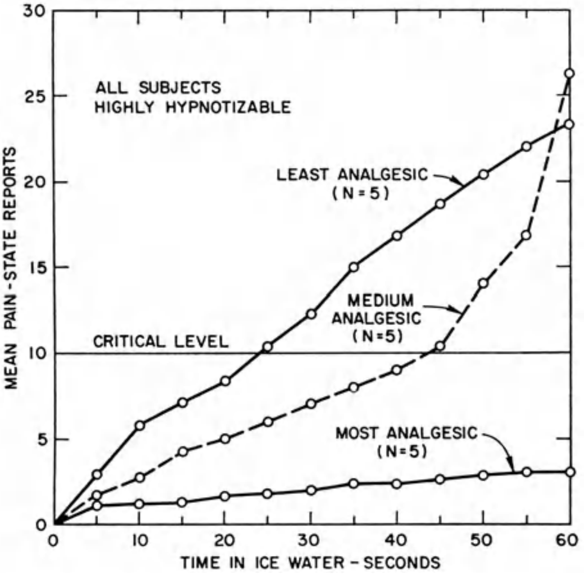

Fig. 2. Differences in pain reduction among the highly hypnotizable (Hilgard, 1967)

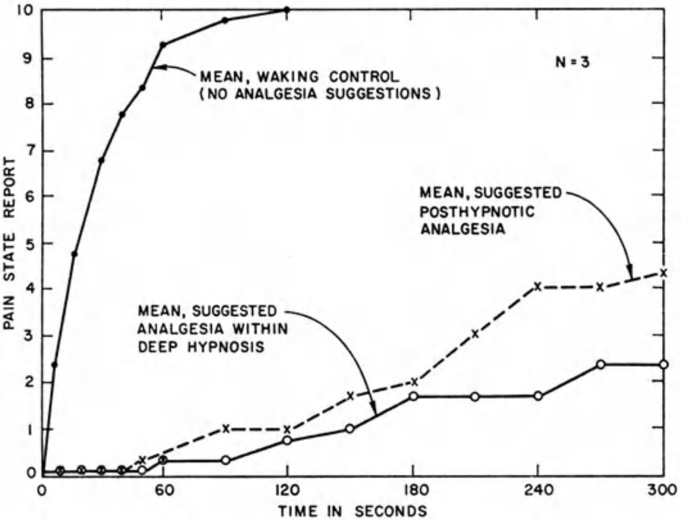

Fig. 3. Control of pain through posthypnotic suggestion (Hilgard *et al.*, 1967)

We may now turn to the physiological correlates of the felt ice water pain, and to what happens to these measurements under hypnotic analgesia.

While there appears to be some reduction in heart rate under hypnotic conditions, it appears unrelated to the amount of felt pain, and may be a consequence of relaxation. It turns out that those who are best able to

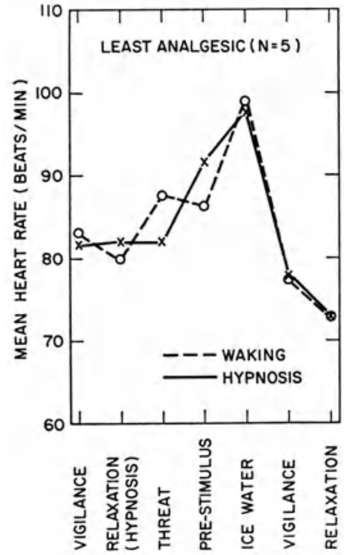

Fig. 4. Heart rate: highly susceptible who cannot control pain (HILGARD, 1967)

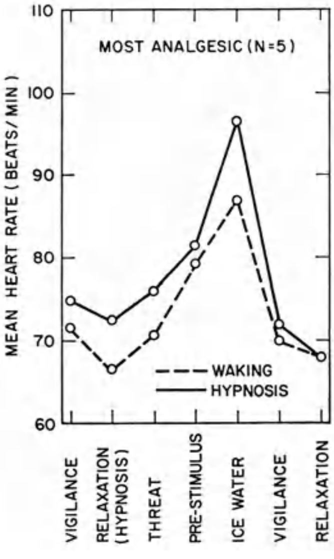

Fig. 5. Heart rate: highly susceptible who can control pain (HILGARD, 1967)

relieve pain do not show the greatest amount of heart-rate reduction (Figs. 4 and 5). Hence the findings are essentially ambiguous.

Essentially ambiguous results were found with breathing also. One of the striking differences between the heart and the breathing reactions was the pronounced effect of the vigilance condition on breathing; thus attention increased respiration about as much as placing the hand in ice water. While breathing may be a weak indicator of analgesia, we do not find any very striking differences attributable to hypnotic analgesia.

A Simulator-Real Comparison, Using a Blood Pressure Measure

Results such as those reported, in which subjective changes in felt pain do not produce corresponding physiological changes, have been used by SUTCLIFFE (1961), and by BARBER and HAHN (1962), to argue against the reality of the hypnotic reduction of pain. SUTCLIFFE attributes the subjective reduction to "delusion", while BARBER tends more to attribute it to compliance with instructions or to task-motivation. We have carried out a subsequent experiment to that just reported, in which we have compared highly susceptible subjects with so-called simulators or role-players who, while insusceptible to hypnosis and suggestions of analgesia, are instructed to try to fool the experimenter into thinking that they are analgesic. This kind of experiment, recommended by ORNE (1962), requires that the "real" hypnotic subjects should be able to control the pain, otherwise the role-players reveal themselves through their consistent verbal denial of pain. The comparison in this case was between responses in hypnosis when analgesia was *not* suggested and when it *was* suggested, thus controlling for the pain reduction associated with hypnotic relaxation.

The detailed precautions in an experiment of this sort are many, and I shall ask your indulgence in believing that we followed them, in order to assure that the experimenter is "blind" as to whether the subject tested was a "simulator" or a "real". We went through a training procedure with all subjects, removing their hands promptly from the ice water if ever they gave a pain report as high as "1". Under these circumstances, both simulators and "real" subjects, having gone through hypnotic induction and analgesic instructions, learned to remain at pain state "0" throughout a minute in the ice water. Fortunately, the ice water is not so intolerable but that a cooperative simulator is able to keep his hand in the water bravely and report that he feels no pain. Students willingly suffer for the sake of science! Under these circumstances, the test of the reality of hypnosis falls back upon the physiological consequences, because the verbal differences have been ruled out by instruction and training.

The added measurement that we have introduced, which we find more satisfactory than heart rate or breathing or pulse volume, is finger blood pressure. I shall describe this briefly, because a later set of figures will also be dependent on it. Plethysmographic records are taken from both the index finger and the middle finger of the right hand while the left hand is submerged in the water. The middle finger also carries an inflatable blood-pressure cuff, pumped up by a servo-mechanism until the pulse from that finger, as recorded from the plethysmograph, is cut off. Then the pressure is gradually reduced until the pulse appears again, and the cycle is repeated. We thus have repeated blood pressure records at about 10 sec intervals. This is roughly a systolic blood pressure, but owing to the complexity of blood responses in the hand and fingers, I am simply calling it finger blood pressure in order not to get into arguments as to what it actually is. By monitoring the pressure at which the pulse begins again, against a threshold defined by the responses from the other finger, we obtain a remarkably consistent measurement. This monitoring is very important, because what should be considered an initial pulse varies as the general plethysmographic record varies. With this digression, let me return to the experiment comparing simulators and "reals". The results are shown in Table 1.

Table 1. *Increases of blood pressure within "analgesia" by simulators compared with "reals" (unpublished results from Stanford Laboratory of Hypnosis Research, courtesy of* JOHN R. LENOX).

Finger blood pressure changes with hand in ice water for 30 sec under conditions of hypnotic analgesia and simulated hypnotic analgesia

Changes in blood pressure between non-analgesia day and analgesia day (mm Hg)	Hypnotizable subjects experiencing hypnotic analgesia	Non-hypnotizable subjects simulating hypnotic analgesia
+10		×
+ 9	×	
+ 6		×
+ 1		×
− 2		×
− 6		×
− 8	×	
− 9	×	
−16	×	
−18	×	

$p = 0.05$, Kolmogorov-Smirnov test

Despite the fact that we have but five subjects in each group, there is a statistically significant difference between them, with the simulators showing greater increases in blood pressure than those genuinely experiencing hypnotic analgesia. Only one of the "reals" showed an increase in blood pressure falling within the distribution of the simulators. Interviews following the experiment revealed that the simulators did indeed feel pain throughout, while the "reals" felt no pain—including the one whose blood pressure went up. Thus the method demonstrates the reality of hypnotic analgesia, but it is not absolutely foolproof as a method of separating role-players from those actually experiencing pain reduction.

It should be noted also that the rise in blood pressure by the simulators is not a function of pain alone, for the rise was accentuated on the day they simulated non-pain, as compared with the day on which they presumably felt equal pain but were free to acknowledge it. We may have some features here of a "lie-detection" experiment.

A Preliminary Report on the Relief of Ischaemic Pain with Highly Susceptible Subjects

Another method of producing laboratory pain, that is somewhat closer to surgical pain than the cold-pressor response, has recently come into favour. This is tourniquet pain, or what has come to be called *ischaemic* pain. We have followed the method used in BEECHER's laboratory (BEECHER, 1966; SMITH *et al.*, 1966). First the arm is milked of blood by wrapping it with an elastic surgical bandage; then the circulation is cut off above the elbow by pumping up the cuff of a sphygmomanometer to 200 mm Hg. The subject now squeezes a dynamometer 20 times and relaxes. The pain mounts gradually, over minutes rather than over seconds as in the ice water. After some time, usually 10 to 30 min, the pain becomes quite intolerable, and if you wish to bring the subject back to the laboratory again you now relieve the pressure. Curiously, there tends to be very little after-effect or pain during the recovery period; the immediate relief is very great. Our experiments are in their early stages, but I wish to present three individual records of highly hypnotically susceptible subjects who have been tested in both the waking state and within hypnotical analgesia (Fig. 6, 7, 8).

Three features of these records are worth noting. First, at moderate levels of pain, represented by pain-state reports of "8" or below (using the familiar 0—10 scale), there is practically no rise in blood pressure. Secondly, once the pain reaches a level that might be described as intolerable suffering, the blood pressure mounts rapidly, along with the mounting pain-state reports. Thirdly, for a highly susceptible subject reporting no pain, the blood pressure does not rise, even though the ischaemia is long-continued, and the same subject gave the normal pain responses to ischaemia in the waking state.

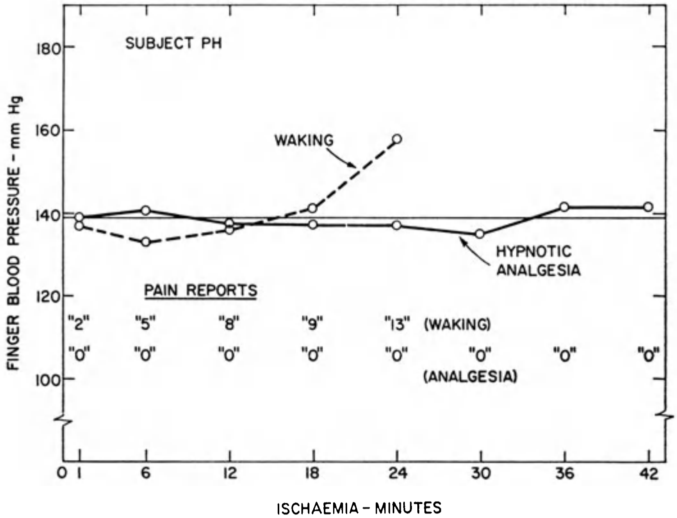

Fig. 6. First subject, blood pressure within ischaemia, waking and hypnotic analgesia (unpublished results from Stanford Laboratory of Hypnosis Research, courtesy of JOHN R. LENOX)

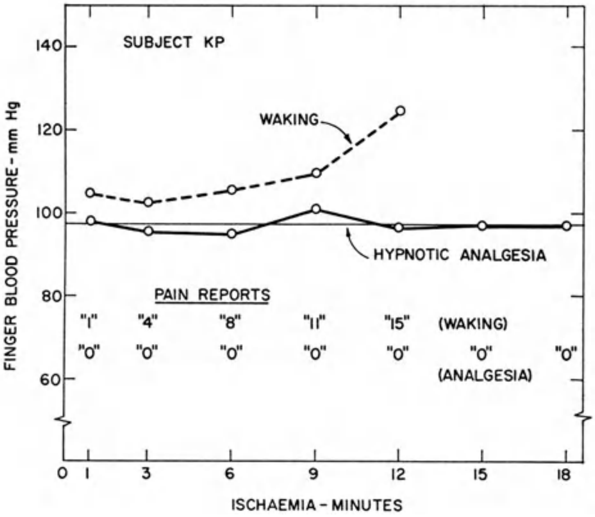

Fig. 7. Second subject, same as Fig. 6

I wish to add a little comment about the problems of adapting formal experimental procedures to somewhat extreme situations of this kind. We have been searching for role-playing simulators who could be trained to endure the ischaemic pain as long as the truly hypnotic analgesic subjects,

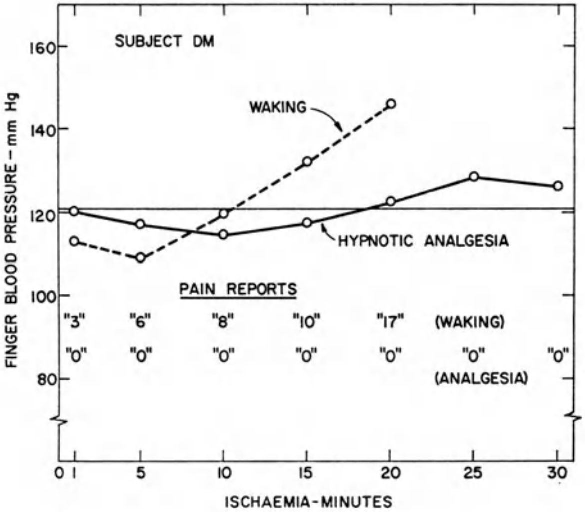

Fig. 8. Third subject, same as Fig. 6

giving reports of "0" throughout, and, by contrast with our success in the case of the ice water experiment, we have thus far not found a simulator who can parallel the hypnotic performance. I suppose we should make the same search for good simulators that we do for good hypnotic subjets, but it puts us under some strain. While what we have already observed is convincing to us as to the reality of hypnotic analgesia, we do not fulfill the formal requirements of the simulator experiment unless we find one who can *act* as if he receives no pain, and thus fools the hypnotist.

Conclusion

Let me conclude by summarizing what I believe to be the present state of our knowledge:

1. First, there is a lack of definite physiological correlates of brief or tolerable pain although the pain is sufficient to produce consistent pain-state reports. This fact accounts for some of the confusion in the literature where brief or tolerable pain stimulation has been used to study the effects of hypnosis.
2. When the pain is of long duration, and reaches levels that can be described as intolerable suffering, physiological changes are consistent, particularly rise in blood pressure.
3. Hypnosis may relieve both the milder pains which do not show specific physiological correlates and relieve the effects of stimulation that would, without hypnosis, lead to intolerable pain and suffering.
4. The actual physiological basis of the pain or its relief remains obscure.

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The Psychophysiology of Hypnotic Susceptibility

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Despite a rich literature of anecdotal and clinical material on the relationship of hypnosis to physiological functions, especially to events in the central nervous system, the research findings are highly equivocal. Most relevant studies have been concerned with shifts in brain wave patterns, as measured by the electroencephalogram (EEG). The studies have attempted to identify the underlying processes which accompany the observed or reported events characteristic of passage between waking and hypnotic states of consciousness. With some exceptions, however, most studies have failed to demonstrate EEG correlates of the hypnotic state (Weitzenhoffer, 1953; True and Stephenson, 1963). Similarly, most studies of physiological functioning have failed to find clear-cut changes in the autonomic nervous system resulting from "hypnosis per se, that is, without further verbal instructions" (Edmonston, 1967).

This paper presents the results of two related experiments conducted independently within a single research program: The first examined electroencephalographic (EEG) patterns as part of the study of imagery in relation to hypnotic susceptibility (London, Hart and Leibovitz, 1968). The second examined thermal and autonomic patterns as part of the study of stress and hypnosis (London and McDevitt, 1967). Both studies demonstrate the strategic importance of accounting for hypnotic susceptibility in attempting to discover the physiological substrata of hypnotic behaviour.

So far, most studies of hypnosis, including most physiological studies, have concentrated entirely on the identification of state differences, and have failed to take adequate account of subject differences which might be critically important to the experimental phenomena. The most important subject variable in this connection is susceptibility to hypnosis. A large number of reliable instruments exist for measuring this very stable trait both

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in children and adults (HILGARD, 1965). Few experimenters have, however, troubled to sort subjects systematically according to susceptibility, nor have they used both high and low-susceptible subjects in the experimental manipulations. This means, in effect, that most hypnotic experiments use only subjects who are very highly susceptible to hypnosis.

A considerable number of experiments on performance differences have shown, however, that high and low-hypnotic susceptibles often differ considerably in operant performance measures of strength, endurance, psychomotor coordination and rote memory. These differences are sometimes further widened or obscured by the application of hypnosis or other experimental instructions to both kinds of subjects. The relationship between hypnotic susceptibility and training or instruction effects is not easily predicted (LONDON and FUHRER, 1961; ROSENHAN and LONDON, 1963a; SLOTNICK and LONDON, 1965). In some cases, indeed, hypnotic training has significantly more effect on relatively unsusceptible people than on highly susceptible ones (ROSENHAN and LONDON, 1963b; LONDON, OGLE and UNIKEL, 1968; LONDON, CONANT and DAVISON, 1966). It is clear, from all this, that the study of hypnotic effects should always take hypnotic susceptibility into account, and this idea was incorporated in the designs of the present experiments.

EEG Alpha Rhythms and Hypnotic Susceptibility*

This experiment was part of a larger study on training in the autoregulation of brain functions (HART, 1967) and the behavioural and physiological correlates of imagery (LEIBOVITZ, 1968). The present report concerns only the observation of differences in the duration of operant alpha rhythm between high and low-susceptibles in two test conditions.

Volunteers for an experiment on "brain waves and hypnosis" requiring 3 h over two different sessions were recruited from college employment offices and through press releases to local newspapers. Of the 154 women, ranging in age from 16 to 61, who volunteered for the experiment, 125 completed the full test battery.

The test battery included: group and individual tests of visual and other imagery; the Harvard group scale of hypnotic susceptibility (HGSS), administered by tape recording; and an individually administered EEG. The group tests were always given first, usually one week before the individual session. The basic pieces of equipment used for the EEG recording were: a Grass model 7, 4-channel polygraph, equipped with 7PI preamplifiers; a Kronhite model 330A bandpass filter tuned to 8—13 Hz. The filter was connected to the polygraph by a lead from J7 of channel 1; a Tektronix model 502A oscilloscope was used for display of the output signal; the amplified output of the oscilloscope was connected to an amplitude-sensitive trigger. The triggering level was established by adjusting the height of the oscilloscope signals to activate the trigger for unattenuated alpha signals, but not for the filter-attenuated signals outside the alpha band. From the trigger, outputs went to a Gra-lab model 172 timer for tuning the alpha duration and to a signal marker on the polygraph which permitted the polygraph operator to check visually the accuracy of the trigger.

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The EEG recording session was divided into ten trials of 2 min in which the subject was told only to relax with eyes closed, followed by a single EEG sample taken over 2 min during a visual imagery task with eyes closed. During the recording session the subject was seated in a comfortable reclining chair inside a large Faraday cage (2.44 m) in a quiet, darkened room.

EEGs were recorded from four sites: channel 1, left occipital to left ear; channel 2, right frontal to right ear; channel 3, left occipital to left frontal; and channel 4, left frontal to left ear. Silver flat disk electrodes by Grass were used; the occipital placement was 2 cm up and 2 across from the inion, the frontal 2 cm up and 2 across from the nasion. Only channel 1 was monitored on-line through the filter-trigger-timer to obtain alpha scores. These scores were read directly in s/sample period of 120 s; these quanta were used as the alpha indices.

For purposes of analysis, HGSS scores were grouped in four categories representing the tenth, twenty-fifth, seventy-fifth and ninetieth percentiles, respectively, of the score distribution. Scores of 0—4 represented low susceptibility, 5—7 moderately low, 8—11 moderately high, and 12, which is the scale's ceiling, represented high susceptibility. Mean EEG alpha durations among the groups (Table 1) were compared by analysis of variance, which showed that the alpha rhythm durations differed significantly both under operant conditions where no special instructions were

Table 1. *Mean EEG alpha Durations (s/120 s) by hypnotic susceptibility scores*

Hypnotic susceptibility Score	N	EEG alpha duration Operant	Visual imagery
0— 4	25	44.5	48.0
5— 7	25	63.9	65.1
8—11	67	50.2	56.1
12	8	84.6	84.6

imposed ($F = 4.89$, $p < .005$, two-tailed test) and in conditions where the subjects were instructed to engage in active visual imagery throughout the period of measurement ($F = 3.30$, $p < 0.05$, two-tailed test). Simple effects tests, as expected from Table 1, further indicated that the most significant differences are a function of the highest and lowest susceptibility groups; the two middle groups do not differ significantly from each other.

Despite the differences in the numbers of trials and instructions, the relationship of alpha duration to scores of susceptibility to hypnosis is almost identical on both the operant and visual imagery conditions. This finding may be of independent interest in its bearing on the controversy about whether deliberate mental activity interferes with the alpha productivity of untrained subjects.

The principal finding, however, that very susceptible people have high alpha durations, gives an important clue about why previous studies have failed to find alpha differences between waking and hypnotic states. Because most experiments use highly susceptible subjects, and because the highly susceptible may routinely produce high alpha under waking conditions, then no change may be observed under hypnosis merely because a ceiling effect is inherent in the subject selection process. In any case, the finding supports the idea that hypnotic susceptibility is a stable personality trait with measurable physiological substrates. Future experiments on differences in state should take these findings into account.

Self-Hypnosis, Stress Response and Hypnotic Susceptibility*

The second experiment reported here originated in connection with a practical problem which is elementary and pervasive in the human struggle for control of the environment, that of how to maintain control over oneself in the face of emergencies so that one's energies can be directed toward the solution of other problems. At the most elementary level, under the most primitive natural conditions, this usually refers to self-control over performance in the face of physical distress like the pain of wounds or the ravages of hunger, thirst, cold or heat. Technically, these are problems of *stress* and *stress resistance* or *tolerance*.

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* This research was initiated by the Environmental Medicine Division, Biomedical Laboratory, Aerospace Medical Research Laboratories, Wright-Patterson Air Force Base, Ohio, with the University of Southern California. The research was conducted in support of Project 7222, "Biophysics of Flight", and Task 72 205, "Biophysics of Flight: Environmental Physiology". This experiment was funded in part under National Institute of Mental Health Career Development Grant No. 1-K3-MH-31, 209—01. PERRY LONDON was principal investigator for the University of Southern California. V. H. THALER, Major, USAF, BSC, Environmental Medical Division, was the original contract monitor and later ABBOTT T. KISSEN, Ph. D. of the Environmental Medical Research Laboratories. This experiment began in July, 1965 and was completed in August, 1966.

The problems of stress resistance also can appear under ideal technological conditions where bodily needs are well cared for. Equipment malfunctions, human errors, or unforeseen "acts of God" may create emergencies where elementary dangers must be confronted by use of elementary skills. Aerospace exploration is one such situation.

This experiment was part of a broad program of studies by the United States Air Force to identify the behavioural and physiological variables which are critical for resisting environmental stresses.

Although there have been a number of studies on the effects of stress on psychological and physiological functioning, there have been few controlled studies on the development of stress resistance. Most stress resistance reports have appeared in the form of clinical anecdotal studies of natural stress situations such as war, internment in concentration camps and physiological trauma (Ax, 1967; Barber, 1959; Bettelheim, 1943; Appley, 1967). Though these studies have much value in their own right, they do not offer the opportunities that might be presented by laboratory experiments for precise examination of the effects of stressful environmental stimuli and resistance efforts on the interactions of physiological mechanisms and psychological variables.

The different types of stimulus conditions that have typically been employed to induce stress can logically be subsumed in two operational categories:

1. The first may be designated as *general* debilitating stressors. These are stress conditions that are characteristically continuous over long periods of time, and tend to influence most measurable parameters of behaviour and function. An example of this type would be thermal stress, such as cold, or prolonged deprivation, such as starvation.
2. The second type may be designated as *interference* stress conditions. These are characteristically short in duration and tend to be task specific in their effects, as would a mild electric shock delivered to one hand, or a loud noise that interfered at random with one's ability to listen to instructions.

The type of measures that have been used to study the effects of stress may also be summarized operationally under two general headings:

1. *physiological* reactions such as heart rate, and basal skin resistance, and
2. *psychological* reactions such as performance decrements on psychomotor coordination tasks, impairments in cognitive functions and subjective distress.

Among general environmental stressors, thermal stress offers an especially good medium for controlled laboratory research, as does electric shock among interference stressors, and hypnosis provides a promising technique for training and examining the processes of stress resistance. Thermal stress has long been the subject of intensive scientific and practical concern, and a few controlled studies relating it and/or pain to hypnotic training have appeared in the literature. KISSEN, REIFLER, and THALER (1964) studied the effectiveness of hypnosis as a stress resistance technique against a general debilitating stressor (cold). They monitored physiological measures including heart rate, basal skin resistance, and shivering rate, in addition to testing psychological performance on a vigilance task. Under hypnosis they found a general amelioration of the psycho-physiological effects of the stress and improvement in vigilance task performance. Similarly, LONDON, OGLE, and UNIKEL (1968) found that both hypnosis and exhortation were effective stress resistance techniques which helped to improve performance on a rotary pursuit task under conditions of generally debilitating stress (heat). SHOR (1962) using a task specific stressor (shock), also used hypnosis as a stress resistance technique and found it to be effective in relieving the subjective experience of pain. All three studies used an experimenter to induce hypnosis just before or during exposure to the stressor. No one, it seems, has studied the development of stress resistance to the simultaneous presentation of both general and interference stressors, or used stress resistance techniques which were in the control of the subject. The latter would provide a better parallel to natural situations in which an individual would have to rely upon his own resources and skills to resist, and it would reduce the confounding of the interpersonal motive of responding to the suggestions of another person with whatever individual motives and capabilities that might operate when the distressed person cannot rely on somebody else's encouragement and help. A self-controlled stress resistance technique, such as autohypnosis, would maximize the generality of the findings and more closely approximate natural conditions.

As ORNE (1965) points out, there have been a few clinical studies using self-hypnosis, but these did not provide appropriate controls (SCHULTZ, 1932; SCHULTZ and LUTHE, 1959). Even so, they do suggest that autohypnosis may be an effective technique for stress resistance.

Stress resistance may be studied by reference to the same criteria that have been used to denote the debilitating effects of stress, namely physiological mechanisms, performance measures and subjective experiences. In this connection, ORNE (1965) points out that it has long been recognized that a deeply hypnotized subject is able to undergo various types of painful procedures with very little objective indication of pain. This observation is supported by numerous accounts of hypnosis as an anaesthetic agent (ESDAILE, 1957; WINKLESTEIN, 1958; MOSS, 1952) in procedures ranging from dental work to childbirth.

A number of similar reports have been published on autohypnosis. SCHULTZ and LUTHE (1959) state that there is a close relationship between traditional hypnosis and autohypnosis or, as SCHULTZ calls it, "autogenic training". Schultz reports a number of cases in which individuals used autohypnosis for reducing the subjective experience of pain or distress, and also claims that individuals using autogenic techniques can be trained to slow their pulse and selectively increase circulation in various limbs.

In a thorough review of the literature on the physiological effects of painful stimulation during hypnosis, Shor (1962) also concludes that physiological responses to pain are less during hypnotic analgesia conditions than during waking conditions. He attributes these findings, however, to the effects of hypnosis in minimizing the anxiety that ordinarily accompanies pain. His point is that most common physiological reactions to painful stimulation, or to stress, are attributable to a secondary psychological reaction (anxiety). By eliminating the anxiety component, hypnosis alters the usually observed physiological response. Shor's work generally supports the findings of other studies that hypnosis is effective in minimizing both subjective and physiological responses to stress (West, Neill, and Hardy, 1952; Miller, Doupe, and Keller, 1939).

Hypnosis and Thermal Stress

Painful stimulation is used in most experiments as an interference stressor. A number of studies have demonstrated how performance is adversely affected by general stress, such as extremes of temperature. In his review of the literature to 1963 inclusive, Ogle lists several such studies (Ogle, 1963). Among these is that by Carpenter (1950), which found that error scores on a pursuitmeter increased as effective temperature exceeded normal (70° Fahrenheit). Vitiles and Smith (1946) also observed that performance on the pursuitmeter shows a decrement when the still air temperature is raised above the comfort range (70° Fahrenheit). Other investigators, using a variety of tasks, have all found a decrement in performance in a heated environment.

The studies cited above, by London, Ogle, and Unikel, and by Kissen, Reifler and Thaler, both indicate that hypnosis has some value for resisting thermal stress as well as pain.

One problem in studying resistance to heat, however, from a physiological point of view, is that it is not obvious how the body is *supposed* to resist. Its facilities for adapting to heat are very restricted and, in some sense oblique—like sweating, which does not cool directly but only if there is ambient air to produce evaporation. Cold stress is another matter, because the body is a natural heat-producing factory, and for limited amounts of cold, its adaptive response is to produce more and more heat. From the point of view of performance deficit, essentially the same thing should be expected in both heat and cold stress resistance, but the advantage to physiological interests suggests that cold stress is a more useful method. Also, since the study by Kissen *et al.* provided a precedent to studying the use of hypnosis as a training method for resisting cold, it was decided to use cold as a general stressor in this experiment.

The main purpose of the present investigation was to study the relative efficacy of a self-controlled stress resistance technique (autohypnosis) in minimizing the combined debilitating effects of general and interference stressors on the physiological and psychological functioning of individuals who differ in their susceptibility to hypnosis. To this end, three specific hypotheses were tested:

1. Imposed stress adversely affects performance and heightens physiological reactions.
2. Autohypnotic training in stress resistance (a) reduces performance deficit, and (b) lowers the level of physiological reaction to the stressors.
3. Autohypnotic training in stress resistance is more effective with subjects who are high in hypnotic susceptibility than with subjects of low susceptibility.

Method

The experiment was performed on sixty-four male subjects ranging in age from 18 to 28 years, all university students, and all in good health.

Subjects were selected from approximately 250 volunteers on the basis of their susceptibility to hypnosis. Half of them were high in susceptibility and half were low. Subjects received three hypnotic susceptibility screenings: the *Harvard Group Scale of Hypnotic Susceptibility* (*HGSS*) (Shor and Orne, 1963), and Forms B and C of the *Stanford Hypnotic Susceptibility Scale* (Weitzenhoffer and Hilgard, 1959, 1962). On at least two of the three they had to score in either the upper or lower third of the scales to qualify for the experiment, and on none of the three could they score at the opposite end. Through this procedure it is possible to obtain a more reliable estimate of susceptibility than is provided by any single measure (Bettelheim, 1960).

An environmental control chamber (cold box), cooled to a mean temperature of 35° Fahrenheit ± 2°, provided the general debilitating stress condition. Painful shock was used as the interference stress. Shock intensity was established individually for each subject and was programmed to be administered at random intervals averaging twenty seconds throughout the performance periods.

Two types of dependent measures were taken: performance and physiological. A standard pursuit rotor task and a special vigilance task provided the performance measures. The vigilance task required the subject to integrate both visual and auditory stimulus signals, paralleling to some degree an airplane pilot's task, especially in executing landings. Subjects were required to memorize a five-digit number presented visually on a movie screen, and then to compare it with another five-digit number that appeared on the screen 3.5 seconds later. If any two digits in the number matched (i.e., if two or more numbers were repeated) *and* if a buzzer sounded while the second number was being observed, the subject was to respond by pressing a key. If the numbers matched but the buzzer did not sound, or if a buzzer sounded but there was not a match, the subject was not to press the key. Errors both of omission and commission were recorded.

Physiological Measures

The following measures were taken:

1. *Heart Rate* (EKG), recorded continuously on a Sanborn 4-channel polygraph.

2. *Basal Skin Resistance*, taken from the sole of each foot by two silver electrodes, and similarly recorded on the Sanborn polygraph.

3. *Shiver and Related Arm Movements.* This variable was measured by a piece of equipment designed and constructed for that specific purpose. It consisted of a suspended piece of plywood upon which the subject rested his unused arm. Movement was detected by use of photocells and was then transmitted into a Sanborn amplifier-recorder.

The instrument was originally intended to measure shiver only, but the stress sessions produced sufficient arm and body movement, to make it impossible to distinguish shivering from somewhat larger movements with any high degree of reliability. Neither kind of movement, however, occurred during non-stress sessions, so that the measure is a direct result of the stress, though not in terms of shivering alone.

4. *Temperature.* Four points of skin temperature were recorded, including the top of the inactive hand (i.e., the hand in the shiverometer sling), top of the foot, chest and back. One point of core temperature was taken, in the armpit. Temperatures were all measured continuously on a Honeywell Temperature Recorder.

Experimental Procedures

After completing the screening sessions, half the subjects in each susceptibility group were randomly assigned to either experimental or control groups and were then scheduled for training on the pursuit rotor. They did not receive any training on the vigilance task, which is not a learning task.

There were three sessions in the environmental control chamber, two base rate sessions (one stress and one non-stress) and one experimental stress session. In all sessions, the subjects were clothed only in undershorts, and were barefoot and barechested. The first base rate session was stressed for half the subjects in each group and unstressed for the other half.

Following the first experimental session, there was an interim of one day to one week before the second base rate session. The average interim period was four days. After the second base rate session, there was an interim of from one to two weeks before the final session.

Subjects in the experimental groups received six hours of autogenic training between the second base-rate session and the single experimental session. Control subjects received no special training or instruction during this period.

Autogenic training was conducted by four expert hypnotists, all members of the Los Angeles Society of Clinical Hypnotists. The training was designed to give the subject control over induction and termination of hypnosis, emphasized the ability to perform with eyes open, and the ability of the subject to cope with the stress conditions and perform optimally on the required tasks.

The experimental session was identical to the first stress session in all respects, except that the subjects in the experimental groups were allowed five minutes to put themselves into a hypnotic trance before entering the cold box.

Scoring Methods

Appropriate scoring systems were devised for both performance and physiological measures, for purposes of data analysis.

The physiological measures were monitored continuously during each session, from the time the subject was completely hooked up to the various electrodes, thermocouples and the shivering apparatus, until the session was completed.

Among the various ways in which the data could most meaningfully be recorded and coded, it was concluded, a time sampling would be most suitable. It was decided, therefore, to record a one-minute reading of each continuous measure and a single reading of each discrete measure at the beginning of each session, and another such reading every fifteen minutes thereafter. Six readings were thus taken during each session and used as the data for each dependent measure, the first reading immediately after hookup, and the others subsequently, after fifteen minutes, thirty minutes, forty-five minutes, sixty minutes, and seventy-five minutes, respectively.

The heart rate measure (EKG) typified the continuously recorded measures. The position on the written record was determined for each of the prescribed time periods, and the number of heart beats during the one-minute time period immediately following that point was the score recorded. Essentially the same procedure was followed for each of the other continuous measures.

The temperature data typified the discrete recordings. It was collected by a Honeywell temperature recorder. The temperature at each of the points monitored (core, chest, back, foot and hand) was printed on the chart at least once every minute. It was thus possible to get a measure of the temperature reading at almost exactly the end of each fifteen-minute time period.

Performance data were generally easier to score than were physiological measures. *Vigilance* was automatically recorded in three error scores: Omission Errors, Commission Errors, and Total Errors. *Pursuit rotor* was recorded as the number of seconds on target for each thirty-second trial, so the mean number of seconds on target for each block of five trials was used as the scoring unit for these data (30/10 data). Performance during the continuous five-minute run (30/30 data) was also recorded in thirty-second units, and for most purposes of the analysis, the score for the entire period consisted of the mean number of seconds on target over all ten second segments.

Results

Strategy of Data Analysis. The design of the experiment made it evident that analysis of variance was the most suitable statistical technique for testing its hypotheses. Since the hypotheses were directed primarily towards differences between sessions and between groups rather than within sessions, it was decided for purposes of analysis to reduce the multiple scores on the eleven dependent variables to a single overall score on each measure for each session. In addition to making the results more manageable and, presumably, interpretable, this would also have the effect of providing a more reliable index per session for each variable, than would be provided by using the multiple scores per session that were available. For the physiological data, this means that the score reported for each variable in a session is the mean of the six scores taken for it within that session. For the vigilance data, the Total Errors alone were used as the session score, since these included both Omission and Commission Errors. For the rotary pursuit data, the session score reported for trial blocks (30/10) is actually the grand mean of all eight trial blocks, and the session score for the five-minute continuous data (30/30) is the same as indicated above, namely, the mean number of seconds on target over all ten thirty-second segments.

Several analyses of variance were performed on all the data of the study, in order to permit exhaustive tests of the hypotheses in terms of main effects, simple effects, and interactions between dimensions. There were two Between-subjects variables, namely, experimental versus control groups, and High Susceptible (T) versus Low Susceptible (UT) groups, and one Within-subjects variable, namely, the three sessions on which all subjects were tested. The main analyses were, therefore (1) a three-way analysis of variance for two Between-subjects variables and one twice repeated Within-subjects variable, to obtain the main effects and interactions for all groups and sessions, and (2) a two-way analysis of variance of the Between-subjects dimensions for each individual session to provide the equivalent of simple effects tests.

In addition, however, it was recognized that the hypothesized effects of stress could be tested separately by comparing the two base rate sessions (the room temperature session and the first stress session) and the hypothesized effects of training could be tested by the separate comparison of the two stress sessions, so (3) three-way analyses of variance for both Between-subjects dimensions and a repeated Within-subjects dimension were conducted separately, to compare the two base rate sessions and again to compare the two stress sessions.

Finally, it was decided that the importance of counterbalancing the order of administration of the room temperature and the first stress sessions, as had been done, should be investigated by comparing the relative effects of session order on performance in the second stress session. For this purpose, (4) a Between-subjects analysis was conducted for the final stress session, with subjects divided according to the order of base rate session administration.

The massiveness and complexity of the results makes it difficult to present them comprehensively and clearly. The presentation will, therefore, focus strictly on the hypotheses of the study and examine the results of all the variables separately for each hypothesis, generally in the order of an introductory summary statement, followed by main effects, simple effects and interactions, and finally by a statement which qualifies the conclusions. First we shall consider stress effects, as in Hypothesis 1, and then training effects, as in Hypothesis 2. Since Hypothesis 3 is a prediction of interactions of susceptibility with stress and training, its results will be incorporated chiefly in the presentations of the other two. The susceptibility effects which were not hypothesized will be presented separately. Finally, we shall discuss order effects which, though not the subject of any hypothesis in this study, are the most important potential source of contaminated results and, therefore, must be examined in order to qualify all conclusions from the hypotheses.

Hypothesis 1: Stress Effects. The first hypothesis of this study, on which the relevance of any training effects depended, was that the stress of cold and electric shock would impair performance and increase physiological reactivity. This occurred without exception on all of the eleven variables under study, and the effects were statistically significant on ten of the eleven.

a) *Physiological reactions.* All the physiological measures except heart rate showed significant changes in the expected direction from the room temperature to the stress sessions. Both on the overall analysis of variance of all sessions and on the analysis which compared room temperature and stress base rate sessions only, the changes which resulted from stress were typically significant at probability levels of the order of 0.005. One-tailed tests were used, since the hypothesis was unidirectional. Though heart rate overall barely changed from the room temperature to the first stress sessions, shivering rate more than tripled, basal skin resistance dropped by almost one third, from 18.58 k to 13.42 k, core temperature dropped from 91.03° to 80.12° (Fahrenheit), chest temperature from 87.88° to 73.94°, back temperature from 89.33° to 73.01°, foot temperature from 83.11° to 62.51°, and hand temperature from 85.88° to 59.85°.

b) *Performance changes.* Changes on the performance measures, while sometimes not as dramatic as the physiological changes, were nevertheless all highly significant. On the vigilance task, Total Errors increased from 26.07 to 38.55. On the rotary pursuit task, where subjects were performing much closer to asymptote to begin with, mean time on target dropped, for the trial blocks, from 24.30 to 21.46 sec, and for the continuous run, from 24.40 to 21.96 sec.

c) *Interactions with susceptibility.* The effects of stress were not always independent of the hypnotic susceptibility of the subjects and occasionally interacted significantly with both susceptibility and training group. In no case, however, were there any significant interactions between the base rate sessions and training group alone. This means that the differences which might later be seen between the experimental and control groups in the final stress session could not be attributed entirely to base rate differences in their reactions.

Although there were no hypotheses about any relationship between hypnotic susceptibility and base rate reactions, the significant interactions which were observed in this connection seem too important to omit mention of them here. Four physiological measures and one performance measure showed such interactions. For the former, UT subjects tend to have higher shivering rates than do T's in the room temperature session (0.399 vs. 0.236), and generally react with more shivering in the first stress session as well. The stress reaction is more pronounced among UT's in the control group than among experimental group UT's, and it is only the triple interaction (susceptibility, training group and stress) which approaches significance ($p < 0.10$, 2-tailed test). There are more pronounced interactions between susceptibility and stress reactions for back, foot and hand temperatures, among the physiological measures, and for the trial-block scores on rotary pursuit, among the performance measures. For both back and foot temperatures, T's and UT's have about the same means during the room temperature session, but T's drop relatively more in the first stress session (back temp., $p < 0.10$, foot temp., $p < 0.05$, both tests 2-tailed). Precisely the opposite happens to hand temperature under stress; *UT's* drop significantly more ($p < 0.05$, for susceptibility × session interaction, 2-tailed test), especially in the control group ($p < 0.01$ for triple interaction, 2-tailed test). On rotary pursuit, however, the differences seem to have more to do with base rate performance under room temperatures than under stress. UT's start with a higher mean performance and, under stress, decline to about the same level as the T group ($p < 0.10$, 2-tailed test).

Summary of Hypothesis 1. In summary, it is very clear that the evidence strongly supports the first hypothesis, but with some qualifications. In other words, it is clear that the stressors had the effects predicted on all groups and almost all measures, but these effects sometimes differed systematically among all the affected groups. Evidently, there are some fundamental differences between people who are high and low in hypnotic susceptibility, which dispose them to respond with different degrees of impairment to the same amounts of stress.

Hypotheses 2 and 3: Training Effects and Interactions of Training and Susceptibility. The second and third hypotheses of this study were that training and self-hypnosis would reduce the performance decrements and physiological reactivity which resulted from the stress, especially among High Susceptibles (T's). These hypotheses were supported in part, but the results in connection with them are considerably more equivocal and complex than was the case with the first hypothesis. Some measures showed a training effect, some did not, and some showed the reverse of the expected effect, both independently and in connection with susceptibility.

a) *Physiological reactions.* Among the physiological measures, heart rate, chest temperature, and core temperature show virtually no mean changes from the first to the second stress sessions among either experimental or control groups. Shivering shows a slight mean increase among control subjects from first to second stress sessions, and a slight decrease among experimental subjects, but it is not possible to attribute this result with much confidence to training. The differences are not statistically significant and, as we have seen, the influences of hypnotic susceptibility and stress on shivering are both so massive that it is difficult for any training effect that might have occurred to be clearly seen. Nevertheless, it is worth noting that the observed increases and decreases in shivering rate are attributable almost entirely to T's, in the control and experimental groups respectively. UT's in both groups stay the same.

Basal skin resistance seems to show more of a training effect than shivering, but this result must also be considered somewhat unreliable. The mean

BSR of control and experimental subjects is almost identical at room temperature, but drops 7.5 points for the experimental group, and only about 2.5 points for the control group, during the first stress session, a difference significant between the 0.05 and 0.10 levels ($F = 3.57$). After training, the experimental group rises a little less than three points and the control group declines a little more than two points, a difference again significant between the 0.05 and 0.10 levels ($F = 3.64$).

Back temperature scores drop significantly overall from first to second stress sessions ($F = 6.66$) by three degrees. Peculiarly, the back temperatures of experimental subjects drop a mean 3.5 degrees after training, from one session to the other, while those of controls drop less than 1.5, but these differences are not significant. Foot temperatures, on the other hand, *rise* significantly from first to second stress sessions by more than two degrees ($F = 10.27$, $p < 0.01$), but they rise more than three degrees among control subjects and barely one degree among experimental subjects, and the difference of more than two degrees between groups in the final session ($F = 4.82$, $p < 0.05$) clearly favours control subjects, precisely contrary to Hypothesis 2.

Hand temperature presents even more peculiar results, in that the mean differences are very slight and very significant at the same time. Experimental and control groups differ in mean hand temperature by barely more than one degree in the first stress session, but this one degree of greater warmth in the experimental group is significant at the 0.005 level ($F = 13.65$). In addition, while training does not have any independent effect, as it inversely did with foot temperature, it interacts significantly if slightly with susceptibility ($F = 6.30$, $p < 0.025$), in accordance with the third hypothesis. As predicted, experimental subjects of high susceptibility rose in mean hand temperature by 0.6 degrees from first to second stress sessions. Unexpectedly, however, though the mean hand temperatures of experimental Low Susceptibles and control High Susceptibles dropped, that of control Low Susceptibles actually rose by more than two degrees. The latter may, however, be a chance fluctuation in the results, and the hypothesis must, in general, be considered supported by the hand temperature data.

b) *Performance changes.* The effects of training on the performance measures are a little easier to describe than were the physiological measures. On all three performance tests, scores increased significantly overall from the first to the second stress sessions (pursuit rotor trial blocks, $F = 39.47$; PR continuous, $F = 18.18$; vigilance Total Errors, $F = 32.98$, all $p < 0.001$).

On the vigilance task, there is a clearly significant training effect in accordance with Hypothesis 2: experimental subjects return to the performance level of their room temperature session after an increase of almost fourteen errors in the first stress session. Control subjects also improve noticeably from the first stress session, but still make considerably more errors than they did originally, and average a significant 7.5 more errors than experimental subjects in the final stress session ($F = 4.25$, $p < 0.05$). Contrary to the third hypothesis, however, the T subjects *are not* more affected by training than the UT's. If anything, the latter group improves very slightly more than the former.

On the pursuit rotor trial blocks, there is no evidence for any training effect. Control and experimental subjects both improve an average of almost three seconds from the first to the second stress sessions. The continuous rotary pursuit data, on the other hand, do show a significant interaction between training and susceptibility ($F = 4.33$, $p < 0.05$) from the first to the second stress session, but this is precisely the opposite of what was predicted by the third hypothesis. Experimental subjects do tend to improve more than controls, but within those groups, it is clearly the UT's who improve most in the experimental group, outperforming all other groups in the second stress session.

c) *Summary of Hypotheses 2 and 3.* In summary, the hypothesized effects of training and interactions between training and susceptibility were not so much wrong as grossly oversimplified statements of what is really the case. Training does have some effects, indeed, but not easily predictable ones. It shows no effects on heart rate, chest temperatures, and core temperatures, and its evident effects on shivering and basal skin resistance, though tending in the direction of the hypothesis, are not very dependable. Back temperatures and foot temperatures, to the contrary, seem to be affected *negatively* by training, the latter reliably so, and none of the training effects indicated thus far interact with susceptibility at all, contrary to Hypothesis 3.

Hand temperature results do support the third hypothesis, though there is no evidence of any independent training effect on this measure, and the differences between groups, though significant, are very small. Vigilance, on the other hand, shows a very clear training effect, in accordance with the second hypothesis, but no interactions with susceptibility, as postulated by the third. Finally, continuous rotary pursuit data show a training effect interacting with susceptibility, but it is *low*, *not high*, susceptibility that benefits from training—and rotary pursuit trial blocks show no significant effects at all.

Additional Results: Hypnotic Susceptibility. In formulating the hypotheses of this study, it was assumed that susceptibility to hypnosis would be a pertinent factor in the benefit derived from autohypnotic training, but that T and UT subjects would be essentially the same with respect to everything other than training effects. As we have seen, this was not the case. Not only did susceptibility interact with the base rate measures, as discussed in connection with Hypothesis 1, but it frequently showed significantly independent effects, often among the most powerful ones in the entire study.

Analysis of variance of all groups and sessions combined shows that hypnotic susceptibility is independently and significantly influential on at least four of the eleven measures studied, and tends to have some effects in connection with another five. Overall, T subjects have lower shivering rates than UT's ($F = 7.11, p < 0.025$, 2-tailed test), lower back temperatures ($F = 6.01, p < 0.05$, 2-tailed test), and higher hand temperatures ($F = 12.58, p < 0.01$, 2-tailed test). Their chest and core temperatures also tend to be lower than those of UT's, but not reliably so ($Fs = 3.41$ and 2.39 respectively). In addition, the mean heart rate of High Susceptibles is 3.5 beats per minute faster than that of Low Susceptibles, and their mean basal skin resistance is almost 3k higher, but neither of these differences is significant ($Fs = 2.66$ and 2.47 respectively). Finally, High Susceptibles tend to perform more poorly on rotary pursuit than do Low Susceptibles ($F = 3.51, p < 0.10$, 2-tailed test), but this difference is seen only on trial blocks, not on continuous performance.

Analysis of the individual sessions generally confirms the overall results and sometimes sheds more light on them. Differences in shivering rate, for example, are seen most clearly during the room temperature session, where T's have a mean score of 0.24, and UT's one of 0.40 ($F = 12.60, p < 0.01$, 2-tails). Differences among the two susceptibility groups in back temperatures and foot temperatures are most pronounced in reaction to the first stress session ($Fs = 5.49$ and 11.19 respectively), but are almost identical in the room temperature session. Differences in hand temperatures also are virtually nil in the room temperature session, are most pronounced in the first stress session ($F = 11.65$), but are still clearly significant in the second stress session, quite apart from any training effects ($F = 9.08$). Finally, the differences in rotary pursuit trial blocks are greater during the room temperature session than at any other time ($F = 6.68, p < 0.05$, 2-tails), virtually disappear under the impact of the first stress session, and tend to recur during the second stress session ($F = 3.45$).

Discussion

The principal purpose of this study was to examine the effectiveness of autohypnotic training as a stress resistance technique; the first and third hypotheses, regarding stress and susceptibility, while important, were formulated primarily in order to gain a more sophisticated understanding of the effects of such training. Consequently, it is more meaningful to discuss the results from the perspective of training effects per se.

The results relating to training effects were more ambiguous than were the very clear-cut disabling effects of the stressors and the different influences of susceptibility on different measures. Certainly training did have a strong positive effect on vigilance task performance, which from a practical point of view is probably the most important measure in the entire experiment, but the effect of training on all the other measures is more puzzling. It seems to benefit continuous rotary pursuit, but only among unsusceptible subjects. Seven of the physiological measures are either unaffected by training, or so variably affected that the results are not reliable. And on one of the temperature measures, training effects appear to be reliable backwards — some subjects' foot temperatures rise after training in how to resist cold, but other subjects' foot temperatures rise even more if they receive no training at all.

In considering these effects, it is important to note that the influence of training is inevitably limited by the magnitude of the stress effects. Considering that stress has a very profound effect on the reactions of most subjects, it is not surprising that the limited amount of training in stress resistance provided in this study does not unequivocally reduce the functional decrements.

Two additional factors which contributed to variation in training effects must also be taken into consideration. These are: (a) Variations in the performance of the expert hypnotists, and (b) Differences in response dispositions resulting from susceptibility.

a) *Expert Hypnotists.* The design of the study was such that, within fairly broad limits, the expert hypnotists were free to use whatever techniques they thought would be most effective, both in training people to hypnotize themselves and in making suggestions for resisting the stress effects. The experts did indeed take advantage of this latitude, and differed from each other in their approaches to the subjects and in the kinds of suggestions they made. Though all of them were very experienced in the clinical use of hypnosis, it is well known that clinical techniques differ rather widely, not so much in induction "patter" as in connection with the concrete suggestions for producing special effects. The considerable differences among the expert hypnotists inevitably contributed an unknown quantum of variation to the subsequent reactions of the subjects.

No effort was made to analyse formally the differences between the hypnotists for two reasons: (a) The dimensional complexity of the study was already so great that it would have been impossible to provide a meaningful analysis of variance design that incorporated both hypnotist differences and the other main effects in which we were interested, and (b) It was recognized that the most critical hypotheses to be scrutinized in this experiment were concerned with whether *any* autohypnotic training would be effective, and not whether stylistic differences in training methods would make a difference.

It may be asserted, for that matter, that the design of this experiment actually tested only whether *training* mattered, not whether autohypnotic training as such was important. To test the latter, it would be necessary to have two comparison groups—one which received no training at all and one which received some specific, but *not* hypnotic training. The economic limitations of the study made it more critical at this juncture to test whether autohypnotic training worked better than no training at all, rather than whether it was relatively more effective than other types of training. Certainly the results are clear enough to indicate that, as expected, it is effective, and future research could legitimately be concerned with the question of type and style of training, comparing different training methods as well as differential effectiveness of different individuals using the same method.

b) *Response dispositions and susceptibility.* Differences in response dispositions related to hypnotic susceptibility were also a major factor in blurring the effects of training. In fact, it is clear that, next to the debilitating effects of the stress situation, susceptibility differences were the most powerful and reliable findings of this study. With respect to performance variables, these findings reconfirm a large number of quite unexpected and still unexplained performance differences between high and low susceptible subjects. The most interesting finding in this connection is the rotary pursuit performance difference—the relatively better performance of UT's both in base rate and stress sessions essentially replicates the earlier finding of LONDON, OGLE and UNIKEL (1968). But their results were obtained under conditions of severe heat stress, whereas the present study used cold stress. Evidently, whatever dispositions of the subjects are reflected in high or low susceptibility produce similar response patterns to hot and cold thermal stressors, even though heat and cold may produce dissimilar physiological reactions. At all events, it seems reasonable to conclude that the similarity of relative performances under these different circumstances reflects a reaction to the common feelings of discomfort which different kinds of thermal stress produce, whether or not there are different physical reactions involved (WEYBREW, 1967, p. 336).

Of even greater interest, and perhaps importance, is the fact that there are significant physiological differences between High and Low Susceptibles, both in base rate performance under room temperature conditions, and in response to the impact of stress. The differences which we had hypothesized in physical reaction were all straightforward derivatives of the assumption that High and Low Susceptibles are fundamentally the same kinds of people in all respects except their susceptibility to hypnosis, so that differences between them could be expected to occur only in connection with their differential abilities to make use of hypnotic suggestions.

Apparently, this notion is far from accurate. The physiological evidence of this study, whether taken alone or pooled with the peculiar performance differences that have been observed in so many previous experiments, seems to indicate overwhelmingly that T's and UT's are in some critical respects fundamentally different people, and that these differences, reflected so strikingly on tests of hypnotic susceptibility, are not fundamentally related to hypnotic experiences, but to a wide variety of life experiences towards which these people respond in subtly, but measurably, different ways.

Whether or not this will prove to be the case in future research, it is quite clear that reactions to hypnotic training cannot be predicted merely from knowing hypnotic susceptibility. Failure to distinguish subjects in terms of this variable, on the other hand, may actually obscure training effects. Without a doubt, all research on hypnotic effects should take into careful account individual differences among subjects, and should evaluate hypothesized effects in terms both of high and low hypnotic susceptibility.

Table 2. *Mean physiological and performance scores by high and low susceptibility groups for all experimental conditions*

	Session		
Measure	Room temperature	Stress 1	Stress 2
Heart Rate			
High	81.31	81.12	81.54
Low	76.92	79.42	76.60
Shivering			
High	0.236	0.921	0.865
Low	0.399	1.061	1.093
Basal Skin Resistance			
High	20.88	13.44	15.54
Low	16.28	13.41	11.97
Core Temperature			
High	90.64	79.14	80.03
Low	91.41	81.09	81.75
Chest Temperature			
High	87.39	73.15	72.47
Low	88.38	74.73	74.67
Back Temperature			
High	89.14	71.28	69.72
Low	89.51	74.74	71.51
Foot Temperature			
High	82.80	60.81	64.19
Low	83.42	64.22	65.20
Hand Temperature			
High	85.99	61.46	61.23
Low	85.78	58.24	58.28
Vigilance			
High	24.09	37.19	27.12
Low	28.06	39.92	27.59
Pursuit Rotor (Blocks)			
High	23.29	21.32	23.51
Low	25.31	21.59	25.03
Pursuit Rotor (Continuous)			
High	23.82	22.26	23.96
Low	24.98	21.66	24.51

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On the Nature of the Posthypnotic Suggestion*

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Attempts to Define Hypnosis

A number of colleagues have discussed various aspects of hypnosis but, for the most part, we have tended to avoid defining operationally what we mean by hypnosis. Often we may not be aware that we have avoided doing so because of the comfortable knowledge that investigators, clinicians, and even laymen will manifest a high degree of consensus about what constitutes hypnosis. Certainly, when an individual agrees to be hypnotized and the hypnotist has carried out a hypnotic induction procedure and then demonstrated that the subject becomes essentially unresponsive to environmental stimuli other than the hypnotist's voice or appropriate suggestions—seems unable to open a clenched fist or bend a straightened arm, is able to sniff a bottle of ammonia with obvious expressions of delight because he has been told the odour was that of a rose, remains relaxed and comfortable despite the application of noxious stimuli, hallucinates objects that are not there while failing to see other objects which he has been told are absent, and, finally, having been told that subsequent to awakening he would nod if the hypnotist removes his glasses, awakens with no apparent recall for any of the events that have transpired yet nods, apparently without awareness of his behaviour, each time the hypnotist removes his glasses—we would all agree that the subject had been deeply hypnotized.

Much of the confusion about hypnosis stems from the fact that, while each of us has a clear picture of the classic somnambulistic subject, there is much less consensus about what constitutes an appropriate working, operational definition with which to carry out our research. The problem is in some ways analogous to the difficulties which for a long time centred

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around the definition of sleep. Again, each of us could comfortably say sleep is that state which we enter each night. Consensus could easily be established when an individual was essentially unresponsive to outside stimuli, his breathing, heart rate, and other metabolic functions were somewhat slowed, his spontaneous activity greatly diminished, and, when aroused from this condition, would report having been asleep. These problems were simplified in some ways when EEG criteria for sleep were discovered. While these criteria were of great help, it was soon discovered that there were several different stages of sleep, and that the relationship between these stages was extremely complex. Not only would different measures of arousal, such as the electrodermal response and EEG criteria, relate in peculiar ways but, given clear physiological criteria of sleep, it seemed that these were not invariably associated with the behaviour which normally characterizes sleep. Natural somnambulism, where the individual may interact with his environment, occurs in what otherwise seems to be sleep's deepest physiological level—Stage 4. Studies in our laboratory, for instance, show that an individual during Stage 1 sleep is capable of carrying out apparently purposive behaviour in response to suggestions administered while he is asleep without any evidence of physiological arousal (EVANS *et al.*, 1966; 1970). In other words, the physiological criteria by which we now commonly define sleep allow us to recognize that it is possible for the individual to be in physiological sleep and yet show patterns of behaviour that we would have heretofore associated with the waking state. It is not intended here to review these fascinating apparent paradoxes emerging from current sleep research: I wish only to point out that, even when apparently clear-cut physiological correlates of a universally recognized state such as sleep become available, not all definitional problems are automatically resolved.

Unfortunately, in the study of hypnosis, we are in an even more difficult situation because no reliable physiological correlates have thus far been identified. Not only do we lack physiological correlates that are associated exclusively with hypnosis, we do not even have physiological correlates that are invariably associated with it. It may be hoped that reliable neurophysiological correlates can ultimately be developed. Until this is possible, it is necessary to utilize behavioural correlates. Traditionally, there are three ways in which hypnosis has been defined, both clinically and in research contexts: (1) By the presence of an induction procedure—hypnosis is the state which ensues after a trance induction procedure has been carried out; (2) by the behaviour of the subject—the subject is hypnotized if he responds to a set of test suggestions; and (3) by the experiential report of the subject—the subject is hypnotized if subsequently he reports that he had been hypnotized. When, as in the example which I gave earlier, all three of these bases for asserting that the subject is hypnotized are included, one can see why consensus about the presence of hypnosis would be so easy to achieve. Difficulty is encountered when different workers focus upon different aspects of the total situation as the reason for asserting that the subject was hypnotized. Each of these criteria has been used by different investigators and each way of defining hypnosis has certain advantages and disadvantages. Most important is the fact that very different empirical results will often be obtained, depending upon how hypnosis has been defined.

Traditionally, the clinician considers as hypnotized (thus ascribing any beneficial results to hypnosis) any patient with whom he has carried out a hypnotic induction procedure and who manifests a modicum of relaxation. Somewhat paradoxically, some of the recent studies carried out by radical behaviourists in the United States (see, for example, BARBER, 1965) follow this identical model; that is, hypnosis is operationally defined by the fact that the experimenter has carried out a trance induction procedure, regardless of the subject's response. Thus, BARBER always views hypnosis as an independent variable in his research. The difficulty with such a definition is that response to a hypnotic induction procedure follows a normal curve of distribution, which means that some individuals will manifest the two other defining characteristics of hypnosis (being able to respond to suggestions and reporting that they have been deeply hypnotized), other subjects will not be affected by the procedure at all, while the bulk of individuals will be affected in varying degrees. Since only some subjects will be hypnotized by the procedure in the sense of showing all the classical characteristics, any effect which would necessarily depend upon the totality of the phenomenon could easily be obscured in any experiment by the large number of subjects who are not responsive*.

Another way of defining hypnosis is by the subject's response to a series of test suggestions. This way of characterizing hypnosis has long been utilized. The Stanford Hypnotic Susceptibility Scale, Forms A, B, and C, developed by Professor HILGARD and his associates (WEITZENHOFFER and HILGARD, 1959; 1962), are a series of carefully standardized tests of such suggestions with well established psychometric properties. Let me emphasize that it is the response of the subject to these test suggestions that determines whether or not he is considered hypnotized. This way of defining hypnosis seems far more appropriate as long as we keep in mind that the behaviour is meaningful only in so far as it truly reflects the subjective experience of the individual. When, for example, the suggestion is given in Stanford Hypnotic Susceptibility Scale, Form A, that there is a fly buzzing around the subject's face and it is troublesome, a facial flinch is scored as a positive response. To the extent that such a flinch reflects the subject's experience of being troubled by a fly, it is an excellent measure of hypnosis; to the extent that the subject may feel pressured to behave in this fashion without actually experiencing the fly, it would represent an error of measurement*.

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* For an excellent discussion of BARBER's work on this issue see HILGARD (1965).

Finally, the extent to which the subject feels affected is the traditional clinical way in which hypnosis is often evaluated. Only relatively recently have investigators focused upon this attribute. The relationship between the extent to which a subject feels affected and his ability to respond to hypnotic suggestions is by no means perfect. Some subjects who, to the observer, seem deeply hypnotized are barely affected; others who are only minimally responsive to suggestions rate their experience as deep and seem profoundly moved. A great deal of work will be required to clarify the extent to which the subjective experience of having been hypnotized may play a role in determining hypnotic behaviour.

In our work we have felt it essential to define the presence of hypnosis on the basis of all three criteria, since only in this fashion can we be certain that we are dealing with the phenomenon that all of us would agree is hypnosis. Starting out in this fashion, it then becomes possible to ask: what aspects that are commonly associated with hypnosis require all three components to be present? It is certainly possible, even likely, that much that is attributed to hypnosis and would be associated with the classic phenomenon may require only one or another of the definitional attributes.

Posthypnotic Behaviour

I would like to report briefly a series of studies which attempt to clarify the nature of posthypnotic behaviour. We chose posthypnotic behaviour in this context because it is one of the behavioural phenomena that, to layman and professional alike, most clearly delineates hypnosis from other kinds of occurrences. Remarkably few studies dealing specifically with the mechanisms of posthypnotic suggestion have been carried out despite the fact that it characterizes the unique qualities of hypnosis.

A deeply hypnotized individual, instructed during hypnosis that at some later time he will carry out an item of behaviour in response to a specific cue, will tend to do so, often without awareness that he is carrying out a suggestion. In fact, posthypnotic suggestion has often been used as one of the demonstrations of unconscious motivation (e.g., ERICKSON'S demonstration of psychopathology in everyday life, 1947). Though it is well established that suitable individuals will carry out even bizarre posthypnotic suggestions, and ERICKSON has shown that, in some instances at least, such responses could be elicited even after a period of years by the hypnotist, it has not been shown to what extent the posthypnotic response depends upon the relationship between subject and hypnotist as opposed to the fact that the suggestion is given during hypnosis. Again, the importance of posthypnotic amnesia is by no means clear in this context. DAMASER (1964), in our laboratory, carried out a study attempting to clarify several of these issues.

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* I would like to avoid the almost metaphysical issues about the extent to which one can know subjective experience. It is intended here only to emphasize the reason why behaviour is a useful criterion because, under usual circumstances, it reflects what an individual experiences. This relationship can, however, readily be distorted by the kinds of instructions.

KELLOGG (1929) and PATTEN (1930) had shown that a posthypnotic response may persist for a considerable period of time, but they had also found the somewhat surprising result that other subjects, not hypnotized, given the same instructions would carry out the suggestion for an indeterminate length of time without showing any diminution. These studies had not been controlled for such factors as depth of hypnosis or extent of relationship between subject and hypnotist, nor was there any evidence to show that they employed deeply hypnotized individuals; furthermore, the subject was required to come back to the laboratory each time to be tested by the hypnotist. We wished to know more about the persistence of posthypnotic behaviour away from the hypnotist.

The first task was to design a measure which would yield data about a subject's performance over a long period of time without requiring him to come to the laboratory. It was decided to give subjects a stack of postcards with instructions to mail one to the laboratory daily. This technique did not require the subject to return to the laboratory but, none the less, provided a free operant response yielding an item of data for each day over a long period of time, actual data collection being carried out by courtesy of the post office. Before using this measure for the study of posthypnotic behaviour, it was felt essential to determine whether it was subject to laboratory manipulation. Accordingly a pilot study was carried out to test the effect of different instructions upon the postcard-sending behaviour over a period of time.

In brief, the question asked was: What was the effect of payment presented as simple monetary reward as opposed to the way the payment was perceived by the subject? Student volunteers were solicited by an announcement to participate in a particular psychological study. They were paid $ 2.50 to take a number of psychological tests lasting approximately $1^1/_2$ h. The subjects were divided randomly into four groups. The first group were told at the end of the experiment that they had done very well, but that it would be very helpful if, as a favour to the experimenter, they would agree to carry out a task which would take but a minute or two a day for the next couple of months. After they had agreed to do so, the experimenter gave them a stack of 60 self-addressed postcards and asked them to mail one to her every day, explaining that it was important that only one be mailed each day and that no two could be sent at the same time.

The second group, also at the completion of the testing, were asked exactly the same question as the first group and, after having agreed before knowing what the task was, they were given a similar stack of postcards and the same instructions. In addition, however, they were told that, at the completion of the study, they would be paid 10 cents for each postcard that was sent. Thus, in addition to the $ 2.50 which they were paid for taking the test, they could earn an additional $ 6.00 by sending the 60 postcards, a total of $ 8.50.

The third group were treated exactly as the first group, with the one exception that they were paid $ 8.50 *for taking the test*, thereby equating the amount of monetary reward that the second group could earn, but they were asked to send the postcards as a favour to the experimenter, exactly as the group that was paid only $ 2.50 for taking the test.

The final group was paid $ 2.50 for taking the test and, after they had agreed to do the experimenter a favour, the task was explained to them and they were also told that they would receive 10 cents for each postcard; that is, a total of $ 6.00 for sending the 60 postcards. However, in contrast to the other groups, they were then paid the $ 6.00 in advance after they had committed themselves to sending the postcards.

It was hypothesized that the last group, which had accepted the $ 6.00 as advance payment for a task that they had yet to do, would send the most postcards because, considering the amount of money involved, guilt would most likely be a stronger motivating force than the possible loss of 10 cents. The findings in this preliminary study came out in line with the hypothesis, supporting the contention that the meaning of monetary reward is of considerably greater significance than the actual amounts involved.

Having shown that postcard sending outside of the laboratory was subject to influence by psychological manipulation inside the laboratory, we utilized this procedure to study posthypnotic behaviour. In the experiment itself, three groups of subjects were compared. The first group was hypnotized, and during hypnosis given a posthypnotic suggestion that they would send a postcard each day. They were then given a stack of postcards, told to place them in their pockets*, and instructed to mail one every day. The subjects were then awakened and told the experiment was completed. No further mention was made of the postcards, and they were sent home with the postcards in their pockets.

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* These postcards were "business reply cards", which means that they do not have stamps but do not require postage. This was to avoid putting undue pressure on the subject by giving him cards which had actual stamps on them and yet not requiring him to do anything beyond signing his name and mailing the card.

The second group of subjects were deeply hypnotized, and given the same suggestion in an identical manner, except that they were not required to put the postcards in their pockets at that time. Instead, each subject was awakened and in the awake state it was explained to him that the experimenter wanted him to do something that was very important to her—to send a postcard each day. This group was then given the posthypnotic suggestion, and requested to carry out the same behaviour, in the waking state.

Finally, the last group was deeply hypnotized but not given any posthypnotic suggestion at all. Instead, upon awakening, they were given a stack of postcards and asked to send one every day. This group then had only the awake request.

The three groups were carefully equated for hypnotizability and the subjects were required to demonstrate all of the criteria of deep hypnosis, including amnesia for the period of one week. After the subject had been carefully tested several times and it was established that he was a good hypnotic subject, he would come for a long session during which he was hypnotized and given the suggestion of posthypnotic amnesia, which was not lifted. Returning to the laboratory one week later, he was interviewed by a research assistant about his experiences during the preceding week. This research assistant behaved as though she did not know what posthypnotic amnesia was and, after the subjects said they could not remember what had occurred while they were under hypnosis, they were urged to try because "it only happened last week; you must remember something about what occurred". It should be noted that only a very small percentage of subjects will maintain complete amnesia under these circumstances. More frequently they will recall one item of experience after another from the preceding session. We feel it is very important that this kind of test be carried out by someone other than the hypnotist, since many subjects report more complete amnesia in his presence.

In addition to this group of very deeply hypnotized individuals, another group of moderately hypnotizable individuals was also run, again subdivided into the three conditions outlined earlier.

The overall findings were somewhat surprising; namely, the groups which received the posthypnotic suggestion alone sent fewer postcards than those who received the request in the awake state. This result was in line with the observations of KELLOGG (1929) and PATTEN (1930). The reasons for this finding became clearer when the experience was discussed with the subjects after the completion of the experiment. Subjects who were in the posthypnotic group correctly surmised that the purpose of the experiment was to determine how long a posthypnotic suggestion would last. Even in the very deeply hypnotized group they became aware of the fact that they were carrying out rather unusual behaviour in mailing a postcard each day to a post office box in Cambridge (U.S.A.), and figured out that this must be related to the experiment. The subjects then tended to send the postcards as long as they felt compelled to do so. When their compulsion to carry out the suggested behaviour waned slightly, they did not feel it appropriate to force themselves to continue to send the cards, since to do so seemed to them to be "cheating". They felt that they were being used to determine how long posthypnotic suggestion lasted and that they ought, therefore, to continue to send postcards only as long as really compelled to do so.

The subjects who were in the waking request group, on the other hand, saw the situation quite differently. They felt that it was very important for the success of the experiment that they continue to send postcards as long as possible, and many of them expressed concern that the nice young lady who was carrying out the experiment might not get her Ph. D. if they failed to send the postcards. It is clear that the subjects' attitudes toward their own behaviour differed greatly and, while it is true that the waking request was more effective in eliciting postcard-sending under these circumstances, we must not lose sight of the fact that the remarkable aspect of the posthypnotic suggestion is that the subjects did send a considerable number of postcards.

Of perhaps equal interest in this context is the implication of these data for the naive concept that posthypnotic suggestion can somehow be explained as simple cooperation with the experimenter on the part of the subject. Obviously, if this were the case, the subject responding to posthypnotic suggestion ought to send the same number of, or more, cards than subjects responding to waking requests to do so. That this is not the case strongly supports the view that we are dealing with a qualitatively different kind of phenomenon.

Before describing the next study on posthypnotic behaviour, a digression is necessary to describe one of the special methodological quasi-control procedures (ORNE, 1962; 1969) used to evaluate the validity of experimental manipulations. Particularly in hypnosis research, we have used control subjects who are simulating hypnosis to a "blind" experimenter, in order to evaluate the extent to which the demand characteristics of the situation might be determining the results of the study. The special treatment group consisted of a group of subjects who had been tested repeatedly concerning their ability to enter hypnosis, and where it had been established that, despite good cooperation and several honest attempts on their part to be hypnotized, these individuals had been unable to do so with several hypnotists. Since the ability to enter hypnosis is remarkably stable, the probability of these individuals' subsequently being hypnotized was quite low. These subjects were then instructed as follows:

"During several experimental sessions you have tried to go into hypnosis and I realize that you have been unable to respond to hypnotic suggestions.

In this experiment your task will be quite different. As part of a special procedure, I want you to take part in this study and simulate being deeply hypnotized. In other words, your task will be to work with Dr. X and to convince him that you are in fact an excellent subject and that you have been deeply hypnotized by him. Dr. X knows that some subjects may try to simulate, but he does not know who, and he will obviously not have any idea that you are trying to fake being deeply hypnotized. If he catches on to the fact that you are simulating, he will stop the experiment immediately, so don't feel that you have given yourself away regardless of what you do, since Dr. X would stop the experiment if you had. Other subjects taking part in this experiment are individuals who have had repeated experiences with hypnosis so that they are able to enter deep trance readily, and it is your task to convince Dr. X that you too are an excellent subject. This holds true not only for the hypnotic session but also for any questions he might ask you about the session subsequently. Since the experiment involves two days, it applies until the experiment is completely over. Under no circumstances should you let on that you are simulating. Only when I see you again at the very end of the experiment will you tell me what really happened. Any questions or tests that are given to you during the experiment you are to answer as though you were deeply hypnotized or had been deeply hypnotized. I realize that you may be asked to do things where you might have no idea how a deeply hypnotized subject would respond. Don't let that worry you. Do the best you can. This is a difficult task, but we have found intelligent subjects able to carry it out successfully. Good luck!"

Simulating subjects given these instructions were randomly interspersed with subjects who were in fact able to enter deep hypnosis readily. For this kind of control group to work properly, it is crucial that the hypnotist in fact be blind as to which subjects are simulating. Extensive work in our laboratory has shown that simple observation, even by highly trained hypnotists, is not sufficient to discriminate between simulating and deeply hypnotized individuals, though special test procedures will allow such discrimination. The virtue of such a special treatment group is that it becomes possible to determine what aspects of the hypnotized subject's behaviour could be due to the average individual's prior knowledge about hypnosis, or explicit and implicit cues given by the hypnotist or inherent in the experimental procedure that may communicate what is expected of the subject (e.g., many aspects of the hypnotized individual's behaviour, such as appearing drowsy; speaking in a low monotone; moving in a somewhat retarded fashion; the ability to show remarkable strength as, for example, when told to be entirely rigid; the ability to support the weight of another individual upon one's abdomen while supported between two chairs; the ability to tolerate painful stimuli with little or no evidence of discomfort; etc., etc.). The fact that simulating subjects may successfully mimic the behaviour of deeply hypnotized individuals does not, of course, indicate that the process occurring in these two groups is the same; it merely demonstrates that hypnosis does not in these instances cause a transcendence of normal physiological capabilities, and that the experimental procedure has not been adequate to show a characteristic that is uniquely a function of the hypnotic phenomenon. This is particularly important, since many of the controversies about hypnotic phenomena appear to have been caused by very subtle differences in treatment procedures. The use of simulating subjects provides a relatively easy objective way in which the experimenter may determine those aspects of a subject's behaviour which could have been inadvertently induced by cues from him that might well have been outside of his own awareness*.

It should be emphasized that the failure to find differences between simulating and deeply hypnotized subjects does not mean that the process by which these behaviours are brought about is the same. This is analogous to asking an individual to simulate schizophrenia and comparing him with actual schizophrenic patients. If individuals are able to simulate schizophrenia, one would not conclude either that schizophrenia is therefore simulation, or that the simulator is schizophrenic. Rather, one would conclude that the diagnostic procedure is not adequate to eliminate the possibility of simulation, and one would hesitate to talk about certain behaviours that could only be ascribed to schizophrenics. Much in the same way, the failure to find differences between simulating and deeply hypnotized subjects in any given experiment merely indicates that the experiment is not conclusive, in that an alternative hypothesis may be entertained about the hypnotized subject's behaviour. This is particularly relevant when we are discussing the transcendence of normal volitional capacities or abilities. Simulating subjects have proven equally as important in physiological studies of hypnosis as in behavioural studies. For some reason, we tend to believe that a physiological response is somehow more "real" and less subject to interference by psychological parameters than the behavioural response. To our dismay, we have found that simulating subjects are often able to produce the same kind of physiological responses as deeply hypnotized individuals. (See, for example, DAMASER *et al.*, 1963.)

Some Recent Studies of Posthypnotic Suggestion

An entirely different approach to an understanding of posthypnotic behaviour follows from the work of SEYMOUR FISHER (1954). In an ingenious experiment FISHER suggested to deeply hypnotized subjects that, on awakening, each time they heard the word *psychology* they would scratch their right ear. After waking the subjects, he tested the suggestion by using the word, and was able to elicit the suggested behaviour. At this point, one of his associates came into the room and by innuendo the experiment was terminated. The associate, Dr. FISHER, and the subject entered into an informal conversation about current topics of the day. In the course of this informal conversation at the conclusion of the study, the word *psychology* came up spontaneously. Of the 12 subjects, only 3 responded during this time. After some minutes of conversation, the associate left and FISHER, turning back to his subjects, would by implication continue the experiment and would conspicuously use the word *psychology* in a sentence. Under these circumstances all of his subjects resumed responding by scratching their right ear. When asked about their behaviour during the preceding period, several of the subjects erroneously insisted that they had continued to respond, while others gave very transparent rationalizations. From this study, FISHER concluded that the posthypnotic response is a function of the subject's expectations of what is desired, and would be carried out only as long as he believed the experiment to be in progress.

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* The controversies between the Salpêtrière and Nancy Schools are a classic example of the kind of issue which could have been resolved very quickly had simulating controls been available.

In a recent study (ORNE *et al.*, 1968), we hypothesized that it was plausible that in the FISHER study subjects perceived the original suggestion to be that they ought to respond by scratching their right ear in response to the word *psychology* as long as the experiment continued, since the request to do so indefinitely would not be a plausible one. If this were actually the case, one would expect the subjects to stop responding when they believed the experiment to be over, and this finding would not necessarily have any implications for the persistence of a different kind of posthypnotic suggestion outside of the experimental situation. From our point of view, the FISHER study does not really test whether a subject who is given a clear-cut, time-limited posthypnotic suggestion will carry it out even under circumstances which he perceives to be outside of the experiment, where the hypnotist would be unlikely to know or even care whether the suggestion had been carried out. This question addresses itself to the issue of whether the posthypnotic response is a function of the suggestion given during hypnosis, or whether it is an attempt by the subject to please the hypnotist, depending, as it were, on the ongoing relationship.

An experiment was carefully designed which would require that the subject come to the laboratory on two succeeding days and take a number of personality tests, some while hypnotized. Subjects were informed in advance that they would be required to come on two successive days. The first day, in addition to taking a number of tests which were, in fact, part of another experiment, subjects were given the suggestion in deep hypnosis that for the next 48 h, each time they heard the word *experiment*, they would run their right hand through their hair. It should be noted that this suggestion was legitimized by the fact that subjects knew that they would see the hypnotist on the succeeding day. The suggestion itself, however, was clear-cut and explicit in placing a 48-h time limit on its effectiveness.

The next day, when the subject came to the laboratory the experimenter was informed by the receptionist of his arrival, and on meeting him in the waiting room and walking down the hall to the experimental room, the experimenter would carefully say,“ I appreciate your coming back to the *experiment*”. This test, however, was not the crucial measure of the posthypnotic response.

Subjects taking part in our research are paid for their participation, and it is customary for them to return to the main office at the completion of a study where, after signing a receipt, they are reimbursed for their time. Each subject, on the completion of the first day's tests, returned to the secretary who asked him to sign the usual form, asking whether it would be all right to pay him “now for today's *experiment* and for the next part of the study tomorrow”. The subject's response under these circumstances, in a different room totally outside the experimental context, constituted the first criterion test. On returning the subsequent day, the subject was met by the receptionist who asked whether he was participating in the physiological or the psychological *experiment*—two studies that were in progress at the same time. The subject's response to the receptionist constituted the second test.

The results showed that, using a highly selected group of subjects who were capable of entering deep hypnosis, subjects responded in the waiting room and the main office much as they had in the presence of the hypnotist. Thus, in some instances, since the word *experiment* was used quite casually, subjects did not always seem to hear the cue; on the other hand, the fact that the context was different, the experimenter was not present, and that the test was carried out in a way that appeared totally unrelated to the study, did not seem to interfere with the response. Interestingly enough, those subjects who responded most consistently were those individuals who also were most deeply hypnotized.

One of the methodological problems in this study was whether the test by the secretary was truly perceived to be outside of the experimental session. If, somehow, the way in which the study was carried out allowed subjects to suspect that the secretary was really recording their behaviour for the benefit of the hypnotist, the findings would have little relevance to the extent to which the posthypnotic response generalizes from one context to another. Obviously we would have much rather tested the posthypnotic suggestion totally away from the laboratory. Even if we had tested it in a classroom or dormitory, however, we would still need to ask whether the subject might not have suspected what we were doing. For this reason we ran a special treatment group of simulating subjects (described above), in order to ascertain how likely it might be that subjects could catch on to the fact that the secretary was really a subtle, but none the less crucial, part of the experimental procedure.

The simulating subjects allowed a hard test of the methodological problem of whether the receptionist was perceived as part of the experiment. In other studies we had noticed the not surprising fact that subjects who are asked to simulate hypnosis tend to be unusually suspicious. It is not surprising that a subject asked to deceive someone should be alert lest he be deceived himself. Therefore, the simulating subject provides a particularly rigorous test for the adequacy of the deception that was involved in our study when we made the crucial test of the posthypnotic response in the behaviour with the secretary and in the waiting room. Running both groups of subjects randomly intermixed and without knowing to which group any given individual belonged, we found that, of 17 deeply hypnotized subjects, 6 responded on both days, whereas of the 13 simulating subjects, not one subject responded on both days. These differences are particularly striking, since in the presence of the hypnotist the simulating subjects tended to respond more often, but in his absence the probability of their response was significantly less.

These data would seem to indicate that the posthypnotic suggestion is different in kind from the response to request. This finding is congruent with the observations of KELLOGG (1929), PATTEN (1930), EDWARDS (1963), and DAMASER (1964), all of whom found that the waking request is significantly more powerful in eliciting behaviour over a long period of time than the posthypnotic suggestion. Thus, we were particularly pleased to be able to demonstrate experimentally the kind of circumstances where the posthypnotic suggestion is more effective than a request—if we interpret the behaviour of the simulating subjects as responsive to a request. The deeply hypnotized subject appears to respond to a posthypnotic suggestion given in a way that communicates the intent that it should be carried out over a period of time—regardless of whether the hypnotist is present or whether the context demands a response. (See ORNE *et al.*, 1968; and SHEEHAN and ORNE, 1968, for a further discussion of these issues.)

On a more anecdotal level, many examples can be cited which illustrate this characteristic of posthypnotic behaviour. It was dramatically demonstrated to us in the course of a pilot study. One of our subjects who had taken part in several studies with the laboratory, and with whom we enjoyed a good relationship, was able to enter extremely deep hypnosis. Because the subject was quite familiar with the laboratory, we felt his behaviour would not be comparable to that of more naive subjects, and therefore we tended to ask him to participate as a pilot subject in new studies.

In the context of seeking an experimental model to get at the unique characteristic of posthypnotic behaviour, I asked the subject, who was a biology student, to tell me the name of an animal that would be very unlikely to come up in the course of his work and about which he thought only rarely. He indicated the seal was such an animal. Since he was at the laboratory to take part in another study and would be back to take part in something else several weeks later, I gave him the suggestion when he was under deep hypnosis that, whenever he said or thought the word *seal*, he would run his right hand over his head. We were in the process of searching about for an appropriate suggestion and felt this one could be tested inconspicuously over a period of time. On returning to the laboratory some six weeks later, the subject spontaneously asked the experimenter with whom he was working at the time, at the conclusion of his particular study, whether perhaps he had been given a suggestion the last time he was at the laboratory, that could account for a peculiar red spot that appeared on his forehead. When the subject was directed to me (as is usual in unusual circumstances), I asked him about the quite noticeable red spot, and he first indicated puzzlement as to how it had gotten there; did he seem to understand how it might be related to any experiment—that it might somehow be connected. At that time, during the conversation, I tested the suggestion by bringing up the word *seal*, to which he responded by placing the palm of his hand on his forehead, over the discoloured area, while resting his hand on his head. He then said, "You know, it's a strange thing that you should bring up the seal, because I became very interested in this animal during the past six weeks. I read all the books the library has about the seal, and my wife and I discussed whether we couldn't get a pet seal to keep at home. I found out something about the cost and, while the seal wouldn't be that expensive, the cost of feeding him fish was pretty steep; also, he likes good facilities to swim, which we didn't have in our apartment." Indeed, our subject had become fascinated with seals and had literally read anything and everything available to him about this animal, expressing considerable dissatisfaction about the inadequacy of the library facilities in this respect! The red spot on his forehead turned out to be due to the heel of his hand resting upon his forehead as his fingers played with his hair while he was studying about the seal.

This somewhat dramatic and unexpected response, of course, constituted a considerable distortion of the original posthypnotic suggestion. Clearly, the sudden interest in the seal served a great many dynamic needs of the subject, and the posthypnotic suggestion only functioned as a trigger for the total behavioural pattern that it served. The suggestion was both brought into consciousness and removed, at which time the subject was able to understand much of what had happened. I felt somewhat disinclined to pursue this particular type of inquiry, since it seemed difficult to predict just how a long-term suggestion might be distorted and used to serve other needs. Certainly, the subject's response cannot be viewed as either role playing or simple compliance; on the contrary, it seemed to be in the service of intrapsychic needs. It was particularly satisfying when it became possible to show in a more rigorous fashion the peculiar quality of the posthypnotic suggestion to transcend the context in which it is originally given.

A recent experiment carried out by Dr. NACE and me* explored what might happen to posthypnotic suggestions that are not carried out. This study also is related to an anecdote told by Dr. ROBERT WHITE**. In the course of giving a lecture many years ago, he demonstrated hypnosis with a student volunteer who was told during trance that, prior to leaving the lecture room, he would place a chair upon the desk in the front of the room. The subject was awakened and the lecture proceeded. At the end of the hour the subject left, together with his colleagues, without carrying out the posthypnotic suggestion. That afternoon, while working in his office, Dr. WHITE became aware of a noise in the corridor and, on investigating, Dr. WHITE noticed the student who had been hypnotized that morning quietly entering the now deserted lecture room, picking up a chair, placing it on the desk at the front of the room, and leaving as quietly as he had come. It seemed likely that, while the response to posthypnotic suggestion was usually seen as an all-or-nothing matter, there must be instances when there is a considerable impulse to carry out the posthypnotic suggestion but various inhibitory impulses prevent the execution of the action. For example, in the case of Dr. WHITE's student, there would have been considerable embarrassment had the student felt impelled to undertake an action which made little sense to him and was unusually conspicuous. On the other hand, later on he sought out the opportunity to carry out the behaviour, again to satisfy something within himself rather than something between himself and Dr. WHITE.

An experiment was designed which would try to grasp this type of phenomenon. Accordingly, subjects were hypnotized at three different times by two different investigators, using the Stanford Hypnotic Susceptibility Scales. During the third session when the subject was administered the Stanford Hypnotic Susceptibility Scale, Form C (SHSS:C), the posthypnotic suggestion was given that on awakening, when the hypnotist removed his glasses, the subject would feel compelled to pick up the blue pencil from a pencil box on the desk and play with it.

The very good hypnotic subject would, of course, respond to this posthypnotic suggestion. We reasoned, however, that there would of necessity be subjects where the impulse to carry out the posthypnotic suggestion would not be sufficient to lead to the actual behaviour, and now sought to create a situation where the subject, in order to fill out a questionnaire, would require a writing implement and where he could, without necessarily even being aware of it, choose to use the blue pencil rather than one of the several other pencils present in the box. In other words, a situation was created where an action which had been suggested and had not been carried out posthypnotically could now be carried out in the context of doing something quite different.

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* NACE, E. P., and M. T. ORNE: The fate of an uncompleted posthypnotic suggestion. J. abnorm. soc. Psychol. (in the press) (1970).

** Personal communication.

Accordingly, at the completion of SHSS:C, the hypnotist took off his glasses and, after talking with the subject some moments more and making certain the subject was awake, indicated that there would be just one more questionnaire to fill out and, asking him to sit at a desk where he himself was at the moment, indicated that he was to complete the final questionnaire*. The hypnotist then indicated that he would return and, as soon as the subject sat down in the chair, turned and left the room. The subject was now alone in the room and could use any one of the writing implements in the pencil box, or a pen or pencil that he might have had with him. We predicted that subjects who chose to use the blue pencil under these circumstances would, in their response to hypnotic suggestion, fall between those individuals who had originally carried out the posthypnotic behaviour and those individuals who neither carried out the posthypnotic suggestion nor chose a blue pencil. In other words, we expected to be able to show that the choice of a blue pencil while filling out a questionnaire subsequent to hypnosis would be lawfully related to the other tests of hypnotizability which were carried out earlier both during the same session and during previous sessions, and that the use of the blue pencil under these circumstances constituted a response similar to that of carrying out the posthypnotic suggestion on cue, but that it would require less hypnotic depth to achieve it.

We, of course, took precautions to have pencil wells with a standardized number of pencils and colours throughout the laboratory, in order both to make certain that subjects would not see these as something special for the experiment on the one hand, and to obtain reliable base lines of pencil choices over a period of time. The occasional subject who showed a preference for blue pencils prior to the posthypnotic suggestion was excluded from the study.

We found that the mean hypnotizability scores (as measured by SHSS:C prior to the suggestion) of subjects who followed the posthypnotic suggestion was 8.5, of those who did not originally follow the posthypnotic suggestion but subsequently chose the blue pencil was 7.1, and of those subjects who were apparently unaffected by the posthypnotic suggestion was 5.9. The same relationship could be shown to hold for the hypnotizability of these subjects when measured by the Stanford Hypnotic Susceptibility Scale, Form A, initially administered by the same experimenter, and by Form B given by a different experimenter.

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* Subjects had filled out a number of questionnaires in the course of this study and this request was completely in line with what had previously been asked of these subjects.

It would seem, then, that a posthypnotic suggestion which is not carried out may none the less lead to an action tendency which, given suitable opportunity to do so, will be discharged. This may take place even though the hypnotist is not present and might have no way of knowing whether the suggestion was carried out.

We have discussed posthypnotic behaviour in experimental subjects. All of the former experiments requested items of behaviour which had little or no dynamic significance to the subject. They were selected to be essentially neutral. We tried to show that a posthypnotic suggestion in an experimental context is generally less effective in eliciting behaviour over a long period of time than a simple request to carry out the action; at the same time, that the posthypnotic response is not limited to the experimental setting but appears to generalize to other settings; and, finally, that execution of the posthypnotic response seems, in some subjects at least, to serve intrapsychic needs and results in a tendency toward closure that is independent of whether the hypnotist is aware of the response. It seems that the likelihood that a subject will carry out a posthypnotic suggestion of this kind is closely related to the depth of hypnosis that is achieved, measured by responsiveness to other test suggestions.

Posthypnotic Behaviour in Clinical Situations

In the light of our discussion thus far, it is relevant to relate the response to posthypnotic suggestions given in an experimental context to the clinical setting. It is worth observing that here we are asking a patient to carry out behaviours that others have previously requested and which he has not been able to carry out; for example, giving up smoking, asking a conversion hysteric with a paralyzed right arm to lift it, etc. This would seem in striking contrast to the experimental situation, where a simple request seems more effective. Yet it has never been empirically tested, whether patients asked to carry out behaviour such as taking a noxious pill might do so in response to a simple request by a physician for a longer period of time than if they were told to do so by posthypnotic suggestion. The difference is that we are usually dealing with a task that, for one reason or another, the patient seems unable to carry out.

An interesting finding is that, in the therapeutic context, the likelihood of a patient's responding to a therapeutic posthypnotic suggestion seems almost uncorrelated with the depth of hypnosis that is attained. This observation has been made by many therapists and is certainly substantiated in my own practice. Not only are there many instances of patients who, although deeply hypnotized, have failed to respond, or responded only transiently to a therapeutic suggestion; but , in striking contrast, there are those instances of patients who apparently were unable to respond to any hypnotic suggestion other than relaxation, but who showed dramatic alterations in behaviour following a therapeutic suggestion*. It seems likely that the mechanism by which therapeutic suggestions become effective is only partially related to the mechanisms by which basically trivial posthypnotic suggestions are activated.

In order to make any statement about it, it is essential to be clear about what we consider hypnosis to be. As long as we consider everything to be hypnosis that occurs once a trance induction procedure has been carried out, it is not possible to determine which are the dynamic considerations. As soon as we more rigorously define hypnosis as the ability to respond to a series of graded hypnotic suggestions, however, it becomes possible to do so. In an experimental setting, LONDON and FUHRER (1961), and ROSENHAN and LONDON (1963), have abundantly demonstrated that hypnotic procedures may have a striking effect even on those individuals who do not enter hypnosis; that is, even though subjects were totally unable to experience any of the striking alterations in subjective experience that, in my view, define hypnosis, they nevertheless showed an increase in physical capacity and perseverance following hypnotic suggestions. It seems plausible that many of the dramatic and effective changes in patients following posthypnotic suggestions do not depend upon the patient's entering the state of hypnosis, but rather are a function of expectation and, perhaps, other subtle changes in their environment associated with the suggestions that have been given.

The recognition that the success of therapeutic suggestions may depend upon a wide variety of dynamic considerations, rather than the presence of deep hypnosis of the sort that is consensually recognized by everyone, may help to clarify some of the confusion concerning the nature of the posthypnotic suggestion. Giving a posthypnotic suggestion in a clinical setting involves communicating clearly to a patient what is desired of him by a therapist who is perceived as extremely powerful, if not magical, whose words may have considerable impact not only on the patient but on those close to him. Inevitably such a situation indirectly brings about changes which have little or nothing to do with whether the individual actually enters hypnosis. It seems worthwhile to consider the possibility that there may be non-specific effects of hypnosis which do not require that the subject experience any of the changes of hypnosis except that he believe himself to have been hypnotized, much in the same way as a placebo response may occur after taking a "drug". In a recent study (McGlashan *et al.*, 1969) we have tried to show how there are both specific and non-specific effects of hypnotic suggestions of analgesia and that these effects are in fact additive.

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* It must be emphasized, of course, that it is essential for the therapist to communicate to the patient that he has done well in hypnosis and that he expects the patient to respond to the therapeutic suggestion. Undoubtedly the expression of disappointment on the part of the therapist would tend greatly to diminish the likelihood of such a therapeutic response. It is perhaps because of the recognition that hypnotic depth does not matter, or the fear of communicating disappointment to the patient, that many therapists are disinclined to evaluate hypnotic depth.

Summary

In this discussion I have tried to emphasize the extent to which the way in which we define hypnosis may determine the kind of observations we make. While I feel that a particular way of looking at the phenomenon may be especially fruitful, it seems much more important that we understand clearly how each of us looks upon it. Many of the difficulties in communication arise from our looking in different ways at different things which are called by the same name. Perhaps with more careful attention to details of definition and the precise circumstances of observation we may be able to clarify the kind of controversies that traditionally have been associated with the study of hypnosis.

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Hypnosis and Personality: A Moderator Variable Analysis

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One of the central and compelling facts of hypnosis research is the common failure of hypnotic susceptibility to relate to any measurable dimension of personality. That state of affairs is both surprising and disturbing: surprising, because several generations of hypnosis researchers have laboured long and hard on this problem and have been paid very little for their effort; disturbing because hypnotizability is so stable a trait (HILGARD *et al.*, 1961; HILGARD, 1965) that arguments which rely on notions of error variance in measurement seem unconvincing here. After several decades of research, very little of substance has been found. Even when a significant finding does emerge, it is often not replicable. So pervasive is the failure to find relationships and replicate them, that the literature on hypnosis already contains an article which indicts the lack of correlation between measures of personality and hypnotic susceptibility (HILGARD and LAUER, 1962).

This is not to say that researchers have not pointed to *potential* relationships in their theoretical writings, nor that they have failed to examine aspects of personality in their relationship to hypnotizability that may not yet be measurable (HILGARD, 1965). Despite these efforts, and with due regard to the essentially clinical observations they have produced, it is nevertheless an odd situation that a trait as stable as hypnotizability should yield no stable correlates with other measurable dispositions.

In this symposium, FURNEAUX proposed what is essentially a Hullian drive approach to the relationship between hypnotizability and personality. I wish to suggest a different view that arises from the following observation: when pervasive zero-order correlations are found between a relatively stable phenomenon and other dispositions to which it should theoretically relate, underlying variables may be moderating those dispositions and masking powerful correlations that may, in fact, exist. The situation for hypnosis is not unlike that which characterized the decision-making literature a decade ago. Risk-taking, as an aspect of decision-making, was found to be stable on the one hand, but apparently unrelated to personality dispositions on the other. Yet, from a theoretical point of view, there indeed should have been relations between this aspect of cognition and other personality variables. Moderator analysis, particularly of anxiety and defensiveness, has revealed a matrix of exciting relationships between the disposition to take risks and other personality dispositions (KOGAN and WALLACH, 1964). Taking their inspiration from SAUNDERS (1956), GHISELLI (1956; 1963) and WALLACH's earlier work (1962), KOGAN and WALLACH divided their subjects into high and low anxiety groups and examined the pattern of correlations that emerged between personality and risk-taking for each of these groups. Anxiety, then, served to moderate the relationship between personality and risk-taking. In much the same way, we propose that there may be significant variables that moderate the relationship between hypnotizability and other personality variables, such that unless and until those moderating variables are taken into consideration, the relationship between hypnotizability and personality will consistently be a zero or near zero.

In the present research it was hypothesized that anxiety moderates the relationship between hypnotic susceptibility and personality. This is to say that one pattern of relationships between hypnotic susceptibility and personality would hold for low anxious subjects, and quite another for high anxious ones. Such a suggestion is not novel in hypnosis research. We commonly analyse our data separately for male and female subjects, on the informed assumption that a pattern of relationships that holds for one sex may not hold for the other, and that combining the sexes without conducting sub-analyses may mask or otherwise distort relationships. A moderator analysis simply extends this procedure to other variables—variables that may not be as visible as sex differences are, but are none the less held to be nearly as pervasive and as powerful in their interaction with other personality traits. In the present research, hypnosis was thought to excite one pattern of needs and defences in high-anxious subjects, and quite another in low-anxious ones. The pattern of needs and defences was not hypothesized.

Method

Eighty-eight subjects (44 male and 44 female), the majority of whom were first- and second-year university students, completed this experiment. All were required to participate in order to satisfy course obligations in either introductory or abnormal psychology. They therefore represent a larger spectrum of subjects than is usually captured in a volunteer sample. One additional subject refused to complete the experiment for religious reasons, and her data are excluded.

Subjects were seen on two separate occasions. During the first, they completed the *Edwards Personal Preference Schedule*, (EPPS), which is a forced choice instrument that assesses a variety of personality dimensions (cf. EDWARDS, 1954). In addition, they completed an *Autobiographical Inventory*, (AI), consisting of 100 items which comprise balanced measures of anxiety, aggression, interpersonal warmth, authoritarianism, and "yea-saying". Finally, they completed a brief form on which they indicated their preferences for a variety of experiments. All told, 33 separate measures of personality and attitude were obtained on each subject.

Three to four weeks later, subjects were asked again to serve in an experiment whose specific nature was undesignated. They were seen in groups of 15—20, and then told that the experiment involved hypnosis. Some introductory (and presumably calming) remarks were made to them about hypnosis which were consistent with those found in the *Harvard Group Scale of Hypnotical Susceptibility* (SHOR and ORNE, 1962). The scale, which constitutes a work sample of hypnotic behaviour and has been shown to be a relatively stable measure of susceptibility (SHOR and ORNE, 1963), was then administered to all subjects.

Results and Discussion

The overall relationship between hypnotic susceptibility and personality is shown in Tables 1 and 2. Only those relationships between variables that have bearing on our discussion are shown. Where direct and moderated relationships between hypnotic susceptibility and personality are statistically insignificant they are not shown in these tables.

Table 1. *Direct and moderated correlations between hypnotic susceptibility and personality among* male *subjects*

Variable	Overall N = 44	High anxious N = 21; $\chi = 13.8$; S.D. = 1.9	Low anxious N = 23; $\chi = 7.7$; S.D. = 1.7	Z diff.	P diff.
EPPS achievement	—0.29[b]	0.05	—0.63[d]	2.42	0.02
EPPS endurance	0.09	0.49[c]	—0.32	2.65	0.008
EPPS deference	0.08	0.37[a]	—0.43[b]	2.64	0.008
EPPS exhibitionism	—0.17	—0.36[a]	0.05	1.30	0.19
EPPS affiliation	0.06	—0.32	0.39[a]	2.26	0.02
EPPS abasement	0.08	0.32	—0.24	1.75	0.08
EPPS intraception	0.22	0.43[b]	0.05	1.26	0.20
EPPS heterosexuality	—0.05	—0.35[a]	0.31	2.10	0.03
AI warmth	0.23	—0.18	0.60[d]	2.71	0.006
AI yeasaying	0.11	—0.11	0.35	1.46	0.14

[a] $p < 0.10$, two-tail
[b] $p < 0.05$, two-tail
[c] $p < 0.02$, two-tail
[d] $p < 0.01$, two-tail

Table 2. *Direct and moderated correlations between hypnotic susceptibility and personality among* female *subjects*

Variable	Overall N = 44	High anxious N = 26; $\chi = 13.3$; S.D. = 1.9	Low anxious N = 18; $\chi = 8.1$ S.D. = 1.6	Z diff.	P diff.
Preference for hypnosis	0.41^{d}	0.40^{b}	0.50^{b}	0.42	n.s.
EPPS abasement	$—0.28^{a}$	—0.33	—0.16	0.54	n.s.
EPPS order	0.15	0.58^{d}	$—0.41^{a}$	3.31	0.001
EPPS intraception	0.05	—0.16	0.39^{a}	1.75	0.08
EPPS aggression	0.08	—0.22	0.52^{c}	2.22	0.03

[a] $p < 0.10$, two-tail
[b] $p < 0.05$, two-tail
[c] $p < 0.02$, two-tail
[d] $p < 0.01$, two-tail

In general, the magnitude of these direct relationships is of a low order, corroborating the experience of other researchers in this area. Even such relationships as appear to emerge, as between the need for achievement and hypnotic susceptibility in males, need to be viewed with caution, since their apparent statistical significance can be attributed to the fact that they arise from a large matrix of intercorrelations.

It was our hypothesis, however, that these zero-order correlations may be masking more powerful underlying relationships. Subjects were therefore dichotomized according to their scores on the twenty-item anxiety scale. The scale itself correlated 0.10 (p = n. s.) with hypnotic susceptibility. For males, the overall mean on the anxiety scale was 11.2 (S.D. = 1.8). For females, the mean was 12.3 (S.D. = 2.0). Subjects whose scores fell below the mean for their sex were considered low-anxious, while those whose scores were above the mean were considered high-anxious. Evidence for anxiety as a moderator of the relationship between hypnotic susceptibility and personality is shown in Tables 1 and 2. We examine the net of relationships separately for each sex.

Males

Low anxiety subjects. Hypnotic susceptibility appears to correlate with two clusters of variables among the low anxiety subjects. On the one hand, there are negative correlations with need for achievement, and with endurance, suggesting that for these subjects hypnosis may be a means of attaining grand ends quickly, with little effort. On the other hand, there is a cluster of positive correlations between hypnotic susceptibility and such variables as need for affiliation, heterosexuality, and warmth, and the tendency to be a "yeasayer", and a negative relationship with deference—all suggesting that the susceptible subject is rather open and available in his relationships and may respond to hypnosis from a sense of trust and mutuality. In view of the obtained relationships between susceptibility and needs for achievement and endurance, the quality of trust involved in perceived relationships may be quite passive.

High anxiety subjects. For the high-anxious subject, the relationship between susceptibility and hypnosis is marked by avoidance. Susceptibility correlates negatively with affiliation, heterosexuality, and exhibitionism, suggesting that highly susceptible subjects are "going it alone". At the same time, positive relationships with deference, and with intraception and abasement, suggest that the highly susceptible individual may be experiencing conflictful introspection, for whose resolution he defers to authority.

Females

On the whole, the direct and moderated analyses brought forth fewer relationships for females than for males. A marginally significant relationship between abasement and hypnotic susceptibility suggested that guilt inhibited hypnotizability for females. The moderator analysis showed that the presence of guilt did not operate for high and low anxious subjects. A strong relationship between preference for hypnotic experiments and hypnotic susceptibility has been discussed (ROSENHAN and TOMKINS, 1964) and corroborated (MELEI and HILGARD, 1964) elsewhere.

Three variables—order, intraception and aggression—provide insignificant relationships with hypnotizability overall, but yield suggestive findings in the moderator analysis. For low anxious subjects, aggression correlated significantly and positively with hypnotizability, while marginal negative relationships were obtained with order and intraception. For high anxious subjects, order correlated markedly and positively with hypnotic susceptibility. We are not yet prepared to interpret these findings beyond themselves, except to note that the relationship between aggression and hypnotic susceptibility in low anxious females is a surprising one, and may be worthy of further investigation.

Caution! We noted earlier that the literature on personality relationships to hypnotic susceptibility is pocked with promising findings that have failed to be substantiated. We do not find it inconceivable that the findings offered here might eventually be among them. Indeed, we urge caution in the use and interpretation of these findings, and further effort in examining whether moderating relationships play a significant role in the links between hypnotizability and personality.

At the same time, the presence of so many moderator relationships gives us reason for some optimism. Note that the overall relationships were no different from those obtained in other studies, which is to say, extremely disappointing. Few statistically significant results emerged, and those could be attributed to chance findings that emerge from large matrices of data. The moderator analysis provided not only more significant relationships (which, considering the attrition of subjects available for each analysis, is surprising) but also many significant differences between the correlations obtained for high and low anxious subjects.

Abstract

Direct relationships between hypnotic susceptibility and dimensions of personality have been difficult to find and, when found, difficult to substantiate. It is proposed that such zero-order relationships are masking more powerful underlying relationships and that certain dimensions of personality may moderate the overall relationship between hypnotizability and personality. Data are presented which corroborate the zero-order correlations reported by other investigators in direct correlational analysis. Rich and distinct patterns of relationship emerge when the anxiety status of the subjects is used as a moderator variable. Moderator variable analysis is cautiously proposed as a promising tool for examining relationships between hypnotic susceptibility and personality.

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III. Psychoanalysis

The Nature of the Controlling Forces in the Hypnotic Relationship

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It will be remembered that psychoanalysis and the various psychotherapies involving insight into oneself all started from the use of hypnosis by Josef Breuer in Vienna to treat his now famous patient, Miss Anna O, who suffered from a severe hysterical disorder. Thus, many analysts from Freud onwards have been interested in attempting to unravel the secrets of the hypnotic state and its controlling forces (Breuer and Freud, 1895). These controlling forces, mentioned in the title of this paper, refer to the nature of the unconscious ties that exist between the hypnotist and his hypnotized subject.

Freud's first statement on hypnosis was contained in a footnote in his book, *Three Essays on the Theory of Sexuality* (1905), where he states that the credulous submissiveness of the hypnotized subject "lies in an unconscious fixation of the subject's libido to the figure of the hypnotist, through the medium of the masochistic components of the sexual instinct".

Ferenczi, in his essay on "Introjection and transference" (1909), suggested that the hypnotic state is an expression of the early child-parent relationship, the subject being the small child and the hypnotist being unconsciously regarded as either its mother or father. He differentiates between the "maternal" form of hypnosis, where the induction into the trance state is gentle and persuasive, and the "paternal" form, where the induction is forceful and authoritarian, e.g. the eye-stare technique. The maternal form would be based on love, and the paternal on fear.

Freud's major contribution to theory was in his book, *Group Psychology and the Analysis of the Ego* (1921). He compares hypnosis with the state of being in love, but with the important difference of there being no sexual satisfaction. He describes the relationship as a group of two people, and suggests that in the subject's unconscious, the hypnotist is put in the place of the subject's ego-ideal (superego). However, he felt his explanation insufficient to account for all the observed phenomena.

JONES, in a paper on "The nature of auto-suggestion" (1923) proposed that the hypnotized subject projects his superego on to the hypnotist. Thus the hypnotist has to fit in with the demands of the subject's conscience and morality to a fair extent, and if his suggestions offend too much against them the subject will not obey.

SCHILDER (1922) added the concept of a masochistic identification of the subject with the hypnotist. That is, the subject, by submitting and abasing himself in a masochistic manner, identifies himself with the supposed power and authority of the hypnotist, which he himself has projected on to the hypnotist.

So far, all the theories have been based on erotic submissiveness and identification. With the establishment of the structural theory of the psyche by FREUD, in his book, *The Ego and the Id* (1923), more attention began to be paid to what is now called ego-psychology together with the various concepts of the development of the ego and its functioning. KUBIE and MARGOLIN (1944) suggested that hypnosis did not necessarily involve any hypnotic relationship between two people, as a trance could be induced entirely by physical means, producing sensorimotor deprivation, but that it could be totally accounted for in terms of an ego-regression of functions, with the subject gradually failing to distinguish between himself and the outside world, and reverting to the most primitive infantile state.

This idea is, however, contested by GILL and BRENMAN, in their book, *Hypnosis and Related States* (1959), in which they maintain that the hypnotic relationship, even if physical manipulations are used for the induction, is essential in the hypnotic state, but nevertheless agree that the regression of ego-functions to more primitive modes of behaviour certainly occurs. Thus, two mechanisms seem necessary—one is a sensorimotor reduction, and the other, the controlling forces of the relationship. Furthermore, following their joint paper with KNIGHT (BRENMAN *et al.*, 1952), they are also concerned with the topic of the subject's hostility towards the hypnotist. They studied fluctuations in the depth of hypnosis of patients in hypno-analytic treatment, and observed changes in depth if the material produced by the patient suggested hostility to the hypnotist. They further emphasized the more subservient role of the hypnotist in this respect, and it is in this respect that my own theorizing, following work with patients, has been further elaborated. I had found that when I asked patients in the trance state to free-associate, I noticed that if I interpreted their associations in terms of the negative transference, i.e. of expressions of hostility towards me, the hypnotist, this would usually cause considerable lightening of the trance, and if these interpretations were persisted in, that the patients could no longer be hypnotized into the trance state. This did not occur if I interpreted the positive transference, i. e. expressions of loving feelings towards the hypnotist. The conclusion I drew from this was that the hypnotic state could exist only so long as hostile feelings of the subject towards the hypnotist were not made explicit.

Another observation, not an original one, was that the hypnotized subject only responded to certain suggestions from the hypnotist and different subjects responded to a different series of suggestions. The implication of this is that it is the subject who decides what he will or will not do.

A third observation, this one very subjective, is that during these sessions with hypnotized patients, I as the hypnotist had strong feelings aroused in myself that the patients were controlling me, just as much as the opposite feeling existed that I was controlling them. These are known as counter-transference feelings, i.e. feelings felt by the hypnotist towards the subject.

To account for these three observations, the hypothesis is proposed that the hypnotic state is based on a collusive deception, i.e. a deception as a result of an unspoken secret agreement, that the hypnotist must pretend to the subject that he is omnipotent and all powerful. The subject's response is to project his own super-ego and feelings of omnipotence on to the hypnotist to foster and encourage this pretence, in the manner of a folie-à-deux. But unconsciously, the subject feels that he is forcing the hypnotist into the deception and is thus controlling the hypnotic relationship himself in this subtle aggressive manner. The hypnotist meanwhile passively accepts this situation and indeed glories in it, with feelings of magical omnipotence in his prowess as a hypnotist. An analogy of this situation is the British Constitution. Here there is a monarch who has all the trappings of power with a parliament that swears allegiance and subservience to the monarchy. The true situation is, of course, that the power is really in the hands of parliament, the all powerful monarch being unable to do anything without its consent.

If this hypothesis is correct, then there must exist anxieties in the patient about the outcome of this aggressive attack on the hypnotist. They will be either persecutory, from fear of retaliation by the hypnotist, or depressive, from fear of the guilt engendered by this attack. We well know that there are often spontaneous outbursts of anxiety by patients when in the trance state. It would also help to explain other phenomena that are observed in the trance state, such as the ability for heightened performance, the existence of positive and negative hallucinations, and the ability to recall past memories and abreact the emotions associated with them, to account for the compulsive nature of the subject's response to the hypnotist's suggestions, to account for the patients' feelings when they come out of the trance state. These ideas are developed more fully in my papers, "A comment on the psychodynamics of the hypnotic state" (1963), and "On consciousness, negative hallucinations, and the hypnotic state" (1966).

To sum up, the controlling forces existing in the hypnotic state are markedly ambivalent in nature, i.e. they concern both love and hate. There is both the erotic surrender and submissions to the hypnotist on the one hand, and the aggressive controlling attack on the hypnotist on the other. Yet, in spite of these insights, assuming they are correct, they do not yet adequately explain certain questions, e.g. why different people are hypnotizable to differing degrees, and what is the physical make-up of the deep-trance subject. We await further progress.

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